THE

GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE IV. VOL. VI.

No. III.—MARCH, 1899.

ORIGINAL ARTICLES.

I.—On the Eastern Margin of the North Atlantic Basin.[1]

By Wilfrid H. Hudleston, M.A., F.R.S., F.L.S., F.G.S., etc.

(PLATE III.)

In offering a few remarks on a subject which belongs, in the first instance, to the province of physical geography, it will be necessary for me to point out certain hydrographical details, whilst endeavouring to deduce from these details conclusions having a geological bearing. Oceanography is almost a science in itself, especially if we regard it from a geological point of view, as something more than a mere description of water-spaces and soundings. Ever since the days when the deep oceans were first explored for the purpose of laying the telegraph cables some of the leading facts were made known, and have since become familiar to all students of physical geography. Such a feature, for instance, as the Continental shelf, usually called the 100-fathom platform, which supports the British Isles, the German Ocean, and its backwater, the Baltic, has long since been made clear to us by the cartographer.

The width of the submerged Continental shelf is very considerable within the area of the British seas. It is generally considered to represent a plane of marine denudation sloping gradually outwards towards an 'edge,' which is approximately represented by the 100-fathom contour, although the real edge sometimes lies rather deeper. This 'edge' is an important feature, since it indicates the angle made by the Continental shelf with the *suboceanic Continental slope*, leading down into the oceanic abyss—what the late Dr. Carpenter might have called the rim of the 'Tea-tray.'

It is to this suboceanic Continental slope that I would chiefly direct your attention, so far as it constitutes the eastern margin of the Atlantic basin. Our limit on the south, for convenience, is the 30th parallel of North latitude; and some approximation to the suboceanic slope may be traced, in varying form and with great difference in the angle of inclination, from the west coast of Africa to the north-west of Spitzbergen. Its mean direction is slightly

[1] Read in abstract at the Meeting of the British Association, Bristol, Sept., 1898.

west of the meridian of Greenwich, with a divergence of about 12° of longitude on either side. This direction it maintains for 51° of latitude, or about 3,500 statute miles. From off the north-west corner of Spitzbergen the suboceanic Continental slope trends easterly, passing north of Franz Josef Land and the New Siberian Islands.

The persistent physical feature which I have called the *suboceanic Continental slope* has been variously described as a 'submerged bank,' and even as an 'escarpment,' though it is certainly not likely to be an 'escarpment' in the geological sense of the term. Before proceeding to trace this particular margin of the north-eastern Atlantic basin, it may be of service to consider some of the remarks of Professor Milne with reference to suboceanic continental slopes generally. These are to be found in his very suggestive paper on "Suboceanic Changes."[1]

Just as we owe the beginning of oceanography to the surveys which preceded the laying down of telegraph cables, so we are likely to learn something about suboceanic movements owing to the repairs which these cables require. Professor Milne brought this subject before the Royal Geographical Society in 1897, and his remarks regarding suboceanic continental slopes are exceedingly important. After referring to submarine banks and ridges generally, he points out that we have evidence of sudden changes in the details of suboceanic contours in the steep slopes flanking the submarine platforms which fringe continents and islands. The causes of suboceanic yielding in rocks and sediments are twofold. The most important is what he calls 'bradyseismical' action, represented by secular folding, thrust, or crush; sudden acceleration in these movements constitute earthquakes, which may be accompanied by a line of fault and landslips. A second group of causes is referred to sedimentation and erosion: these create unstable contours, which may be destroyed by seismic action or in other ways. He calculates that one-half of the earthquake shocks occurring throughout the world have their origin in the suboceanic continental slopes. He divides these into *seismic* and *non-seismic* districts. Slopes of considerable horizontal extension—say 120 geographical miles—which are steeper than 1 in 35 ($1\frac{3}{4}°$), are likely to be the seat of submarine earthquakes accompanied by landslips. As examples of seismic districts he instances the west coast of South America, the Kurile Islands, and parts of Japan; whilst of non-seismic districts he instances the coasts of Australia, Scotland, and South Norway.

Seismic strain, due to a tendency in the earth's crust to further adjustment, is greatest where earthquakes most frequently originate; and the special home of the earthquake is at the base of the steep oceanic slopes, where most deformation is in progress. He further observes that the results of vertical movements are those which are now most apparent, but that the *geotectonic structure* of many countries shows great horizontal displacement likewise.

[1] Geographical Journal, August and September, 1897.

It will be gathered from the preceding extracts that Professor Milne regards the suboceanic continental slope as an important factor in the tectonic history of the earth's crust. Looking at the subject broadly, one might say that this suboceanic slope performs a double function. In the first place, as the more or less abrupt termination of a continental mass, it affords relief to the periodic strain, and thus becomes at times the seat of dynamic energies of great force. Secondly, its more obvious function is to contain all but the surface waters of the great oceans within its limits, and it thus becomes the true margin of their respective basins.

Trusting to be pardoned for what might seem at first sight a digression, I will now direct attention to a few hydrographic details with reference to the particular suboceanic slope under consideration. We have already seen that it extends over 51° of latitude. Allowing for sinuosities and for the easterly extension beyond Spitzbergen, we are dealing with a marginal line which cannot be less than 5,000 miles in length. The principal authorities for these details, and also for the accompanying map, are as follows:—

Nansen's paper "On some Results of the Norwegian Arctic Expedition." [1]

"The Physical Chart of the North Polar Regions," by Bartholomew.[2]

"The Norwegian North Atlantic Expedition," by H. Mohn.[3]

And lastly, several of the British Admiralty charts, e.g.:

Chart No. 1, "British Islands to Mediterranean Sea," published in 1884, with small corrections down to 1897.

Chart No. 2, "The British Islands," published in 1884, with small corrections down to 1894 and large corrections down to 1896.

Chart No. 1,104, "The Bay of Biscay," published in 1888, with large corrections down to 1892 and small corrections to 1897.

Chart No. 2,058, "North Atlantic Route Chart," published in 1892, with small corrections down to 1897.

Also other Admiralty charts.

It must be understood that the accompanying map has no claim to accuracy of detail; if a general impression is conveyed, that will be sufficient for the purpose.[4] The 'edge' or angle between the

[1] Geogr. Journal, May, 1897. [2] Ibid., June, 1897. [3] Christiania, 1887.

[4] This remark refers to the large chart which was exhibited at the Bristol meeting. Since then Messrs. Bartholomew have prepared a more elaborate map based upon the outlines of that chart. This will serve to give a *general idea* of the subject of the paper, viz. "The Eastern Margin of the North Atlantic Basin." It will be readily understood that details can only be shown on a larger scale. Indeed, for very close work in connection with the "*suboceanic Continental slope*," it may be doubted whether a sufficient number of reliable soundings have been taken as yet to warrant any detailed mapping of these slopes in some cases. Notwithstanding the imperfection of the record, however, there are some features in the marginal line, such as the 'Fosse de cap Breton,' which have been clearly established (see Fig. 4). It will be observed that a very small area in Messrs. Bartholomew's chart, to the W.S.W. of Madeira, shows depths exceeding 3,000 fathoms. This, however, has no bearing on the subject of my paper.

Continental platform and the suboceanic slope varies as to degree of submergence, being mostly on the 100-fathom contour in the British seas, but nearer the 200-fathom contour off parts of the Norwegian coast.

Part I.—The North Polar Ocean, and the Norwegian Atlantic.

Since the most important recent results in oceanography have been obtained in the North Polar basin, it seems only proper to commence our bathymetrical studies in the far north, and to work our way southwards as far as the limits already indicated. The results obtained by the "Fram" were so contrary to what was generally expected, that it may be some time before their true import is fully realized. They prove beyond doubt, however, that the Atlantic depression, which we knew was continued into the Norwegian Atlantic and thence through the ocean narrows between Greenland and Spitzbergen, expands in the North Polar area, and in all probability extends to the Pole itself on the meridian of Greenwich. Nansen tells us that he had imagined the Polar basin, taken as a whole, to be shallow, but he found the sea north of the New Siberian Islands suddenly becoming deeper and sinking to a depth of 1,800 to 2,000 fathoms, and that this depth was maintained during the entire drift of the "Fram" as far as north of Spitzbergen. He therefore takes it for granted that the Polar basin is a deep sea, which forms a continuation northwards and eastwards of the same depth as the Northern Atlantic Ocean; he concludes that there is little or no land on this side of the Pole, and this conclusion is based on several considerations of great weight.

According to old soundings taken long before the voyage of the "Fram," a depth of 1,370 fathoms is recorded about 100 miles north of Spitzbergen; and this might have been accepted as an intimation of what was to be expected further eastwards. We can believe that the "Fram" crossed a suboceanic slope in about 79° N. lat., and that this slope will be found to extend all along the northern margin of the shallow platform which supports the Franz Josef Archipelago, itself only an eastern extension of the Spitzbergen group.[1] This platform bears the same relation to the north of Europe that the British platform does to the north-west of Europe. The chief difference lies in the fact that the mouth of the Barents Sea, between Bear Island and the North Cape, is deeper than the average edge of the North Sea platform by about 170 fathoms, though the eastern parts of the Barents Sea are shallow enough.

A few of the details of the Norwegian Atlantic may next be taken into consideration. The several parts of this have received different names, but it may be regarded as practically one basin in alignment with the North Atlantic Ocean. The major axis of the principal deeps is for the most part meridional, and, indeed,

[1] Since Dr. Koettlitz records (Quart. Journ. Geol. Soc., vol. liv, p. 644) a depth of 230 fathoms thirty miles west of Cape Mary Harmsworth, it is possible that a bight of the deep North Polar ocean may extend for some distance between the Franz Josef Archipelago and Spitzbergen.

lies almost exactly on the meridian of Greenwich. The most remarkable depth is that known as the Swedish deep, attaining 2,650 fathoms in the narrows about midway between Spitzbergen and Greenland. The average slope of the sea bed here is stated by Mohn to be 1 in 129, or less than half a degree; but such a general statement gives no idea of the inclination of the marginal slopes off the west coast of Spitzbergen. Thus, about 50 miles from the shore, off the mouth of Ice Sound, the edge of the Continental platform is well marked in 120 fathoms, and succeeded by a rapid marginal slope towards the abyss. From Sydkap to Bear Island the suboceanic slope is fairly well marked, the edge being mostly in 200 fathoms. Off the mouth of the Barents Sea the slopes are gentle, the edge being in about 280 fathoms. But as we approach the Lofodens the contours begin to draw in much closer, and the steepest part of the suboceanic slope off the coast of Norway occurs hereabouts (Vesteraalseg). The depth increases from 100 to 1,500 fathoms in a distance of $22\frac{1}{2}$ nautical miles, giving, according to Mohn, a slope of 1 in 16 or $3\frac{1}{2}°$. The general Norwegian submerged shelf, as before observed, has its edge near the 200-fathom contour, which runs rather far out to sea and again curves towards the land opposite Romsdal; on the other hand, the 100-fathom line hugs the coast of Norway pretty closely.

The Norwegian Channel, sometimes called the River of Norway, is a curious feature, whose origin has given rise to a considerable amount of discussion. As a matter of fact it is a depression of considerable width, which laps round the southern lobe of Norway from the Skager Rack to beyond the mouth of the Sogne Fjord. It has also, I believe, been called the 'River of the Baltic,' and regarded as the submerged channel of a watercourse which once drained the Baltic area when that was dry land. Unfortunately for this theory the deepest part of the Norwegian Channel lies in the Skager Rack off Arendal, where a depth of 443 fathoms is recorded by Mohn, whilst the Channel is only 160 fathoms deep off Bergen on the Atlantic side, between Norway and Shetland. The 'River of the Baltic,' therefore, must have had some difficulty in draining towards the Atlantic under these circumstances.

Speaking generally, one would say that the basin of the Norwegian Atlantic is more spoon-shaped than that of the British Atlantic: in other words, the marginal slopes appear to be less steep on the whole. A low transverse elevation runs in a north-easterly direction from Jan Meyen to Bear Island, almost athwart the main meridional depression; Jan Meyen Island itself is a huge volcanic pimple. The steepest decline in the Norwegian sea was found north-west of this island, where the depth reaches 1,040 fathoms at a distance of only seven nautical miles from the land, corresponding to a slope of 1 in 7, or 8°, being the direct continuation of the lava slopes of the extinct volcano called the Beerenberg. On the south side of the island the slopes are more gradual. It is evident that in course of time this volcanic mass must have displaced an

enormous volume of water, and unless there was a corresponding sinking-in somewhere, some perceptible effect must have been produced on the sea-level. I might mention here that in Mohn's admirable work a number of serial soundings are given throughout the Norwegian Atlantic. These of necessity are drawn with the vertical scale much exaggerated. In this way even the central deeps may be made to look like cañons with V-shaped valleys at the bottom. This is especially the case in the deep-sea between Spitzbergen and Greenland, and it is easy to make a slope of 3° or 4° look almost vertical. In the southern part of the Norwegian Atlantic the *average* rise of the sea bed from its greatest depths towards the coast of Iceland, the Færoes, and Norway is stated to be 1 in 188, or about one-third of a degree.

Before proceeding any further south with our bathymetrical survey, we ought to consider the geological bearing of the details already given, as far as practicable. It is obvious from an inspection of the accompanying map that the eastern part of the North Atlantic, the Norwegian Atlantic,[1] and a portion of the North Polar ocean all belong to one and the same great geosynclinal depression, which has been partly interrupted by volcanic extravasation, more especially in the region between Shetland and south-east Greenland. This linear arrangement of what I may call, in general terms, the North Atlantic oceanic depression, extending from the North Pole in the direction of the Equator, seems to call for some explanation on physical grounds. We are reminded of the two principal schools of geographical evolution, and of the theories relating to the permanence or non-permanence of the major features of the earth's crust. Ever since the leading facts of oceanography were made known there has been a tendency in some quarters to regard the great oceans as permanent features. If this is really the case, then the suboceanic continental slopes would, under certain limits, come to be regarded as permanent also.

Dana and Wallace have argued strongly in favour of the permanence of the great features of the distribution of land and water on the earth's surface, but the majority of geologists in England have rather clung to the Lyellian doctrine as to a complete change of land and sea having taken place at various periods of geological time. The secular interchangeability of the great land and water areas has also received much support from the view that the Chalk is of necessity a deep-water formation—abyssal according to some. Wallace,[2] it is well known, considered that the doctrine of the permanence of continents and oceans lay at the root of all our inquiries into the past changes of the earth and its inhabitants. He thought that this doctrine received strong confirmation from the evidence adduced by Darwin, who observed that hardly one truly oceanic island had been known to afford a trace of any Palæozoic or Mesozoic formation; so that islands of this class have not preserved

[1] In Bartholomew's map (Fig. 1) the northern part of the Norwegian Atlantic is called the 'Greenland Sea.'

[2] "Island Life."

a fragment of the supposed ancient continents, nor of the deposits which must have resulted from their denudation.

Unless this reasoning has been upset by fresh evidence, it ought to have considerable weight with geologists. No doubt some of the West India islands, and notably Barbados, afford examples of oceanic deposits of Tertiary age resting upon terrigenous deposits known as the Scotland Beds. Nevertheless, even Barbados lies inside the true suboceanic continental slope which margins that side of the Atlantic, whilst the Caribbean Sea may be regarded as a region of exceptional oscillation, which partakes to some extent both of an oceanic and a continental nature. The Mediterranean in a less degree presents similar features, and it seems to me that no valid argument can be based on events which have occurred or are occurring in such abnormal areas.

Another very specious argument against permanence is derived from the great height to which marine Tertiary beds, such as the Nummulitic, are raised in the Alps, Himalayas, etc. Why may not changes of equal magnitude, it is urged, have taken place conversely in areas now occupied by the great oceans? It should be borne in mind, however, that the upheaval of Tertiary marine beds to these great heights naturally takes place in mountain ranges, where tangential pressure is at a maximum along lines which are of great length but very limited in width. Such features can hardly be compared with the great ocean expanses, whose breadth is measured by thousands of miles. On the other hand, there is a considerable degree of analogy between these prolonged mountain ranges and the suboceanic continental slopes which bound the great oceans. Both classes of phenomena are largely due to tangential pressure, the result of secular contraction. Hence for the most part mountain ranges are believed to occur and reoccur with nearly the same orientation throughout geological time. They form the lines of adjustment, as it were, in the earth's crust, which became especially acted upon at certain epochs; and the fact of marine Tertiary strata having been raised to heights ranging up to 20,000 feet, although it shows oscillation to a great extent within a long and narrow area, does not justify the idea of an equal amount of oscillation in the ocean basins.

The principal region of vertical movement in connection with the oceans is probably to be sought in the suboceanic continental slope, and such movements would affect the shelf or continental platform more than the abyssal floor, which would remain comparatively unaffected. In this way the upper portions of the submarine slopes on the sides of the Atlantic may have emerged at times along the line, more in some places and less in others, thus constituting saliences which were worn back by the planing action of the sea; and in this way the Continental shelf, or rather portions of it, may be of considerable antiquity.

If this view be correct it will allow for a considerable amount of change throughout geological time on the margins of the great oceans without materially affecting the ocean basins themselves.

We may thus regard the crust of the earth as still retaining traces of its congenital features in the shape of the great ocean depths, and also in the position of the chief continental areas which have been built up from deposits formed for the most part in shallow seas, though with some important exceptions. On the question of the primal origin of these features we obtain some valuable physical hints from Lord Kelvin[1] in his remarks on that very critical stage in the earth's history when solidification was commencing on the surface. His view is that continents and ocean depths were due to heterogeneousness in different parts of the liquid which constituted the earth's surface before such solidification. The hydrostatic equilibrium, he says, involved homogeneousness in respect to density only, but none in respect of chemical composition: from this heterogeneousness the irregularities of the present surface, he thinks, followed by dynamical necessity.

No apology is needed for quoting the views of one of the first physicists of the day as to the possible origin of some of the major features of the earth's crust. But it is time, after this diversion, to consider the bearing of Nansen's discoveries in reference to theories of geographical evolution. As already pointed out, the northern boundary of the European continental platform must be sought in Spitzbergen and the Franz Josef Archipelago. At present every detail in connection with this prolongation of the eastern margin of the Atlantic basin is wanting. We know that a shallow sea becomes a deep one, and that is all. But the question arises as to how long in the history of the earth has this feature endured? Is a deep North Polar ocean one of the permanent features of the earth's crust?

There are no people so ready to make light of the problems of geographical evolution as the biologists. Whenever a botanist or a zoologist is desirous of explaining the peculiarities of distribution, he is quite ready to conjure up a continent from "the vasty deep." Thus, it is not so long since we heard of a famous botanist requiring a large land area in the Polar ocean. But in the light of recent discoveries we may feel certain that no such land area exists at present; whilst the great depths recorded by the sounding-lead render it unlikely that land has existed there so recently as to have influenced plant distribution. Even supposing that Greenland belongs botanically to Europe, it is not in the North Polar area that the connection is likely to have been made. Speaking generally, one might say that, since the sea is in possession of the depths, the *onus probandi* rests with those who would alter the condition of things. Apart from mere surmise, it is by no means easy to prove that a water-space has ever been a land area, though of course the converse of the proposition presents no difficulties. We have abundant evidence that, on the continental side of the suboceanic slope, there have been oscillations of moderate amount, resulting in the formation of terrigenous deposits of many ages in connection with what is now the shallow Barents Sea. If any volcanic island, outside

[1] Annual Address to the Victoria Institute for 1897.

the continental slope, could be shown to contain deposits resulting from continental degradation, it would be difficult to maintain that no dry land had existed where now are oceanic depths; but failing such evidence as this, we must be content to regard the deep Polar ocean as a permanent feature.

Again, as a more especial argument for breaking up the existing North Polar basin, it has been pointed out that the strike of the rocks and the trend of the mountain chains for the most part run north and south, ending abruptly on its margin. The idea is that these mountain chains, having what is called a 'Ural orientation,' once extended further north, and have in some unaccountable way been removed or cut short. Undoubtedly, we know so little of the true termination of these highland areas that no theory can safely be founded on their apparent truncation. There are, however, other regions where a mountain range is obviously truncated by the sub-oceanic continental slope—notably the Cantabrian chain in the north of Spain, which may be said to disappear suddenly in 2,000 fathoms of water. As a possible explanation I would suggest that such disappearance is due to tectonic failure in the process of mountain-making, such mountain-making being especially connected with continental elevation. On approaching the margin of an oceanic area another set of tectonic conditions prevails. In this way the mountain range fails from internal causes, and, therefore, any theory accounting for its removal becomes superfluous.

(*To be continued.*)

II.—Notes on the Geology of West Swaziland, South Africa.

By Professor T. Rupert Jones, F.R.S., F.G.S.

MR. SYDNEY RYAN, of Durkton, Managing Director of the Ryan Tin Mines, situated on the Embabaan River in West Swaziland, has collected and sent to England a large collection of rock-specimens from the Ingwenya Berg, the dominating feature of the district. He has also provided plans and sections of the district, which are here partly reproduced. The specimens have been presented to the Mineralogical Department of the British Museum, together with a complete catalogue, having reference to the Sections referred to above, and indications of the lithological characters of the 140 specimens.[1] Mr. G. Th. Prior, F.G.S., has kindly aided the writer in the critical examination of the rocks, and is communicating a description of those of economic value to the forthcoming number of the Mineralogical Magazine. They include niobates and titanates of the rare earths, chemically allied to Euxenite and Fergusonite, Monazite containing Thoria, Cassiterite crystals of peculiar habit in matrix, Corundum, and Scheelite.

Our friend Mr. John Ballot, F.G.S., who knows Mr. S. Ryan and the district very well, has favoured the writer with much information,

[1] The specimens, labelled and numbered, are individually referred to as belonging to the several divisions of the three great sections, X. T, Z, at the end of this paper, with some unavoidable irregularity in the successional numbers. (See pp. 110, 111.)

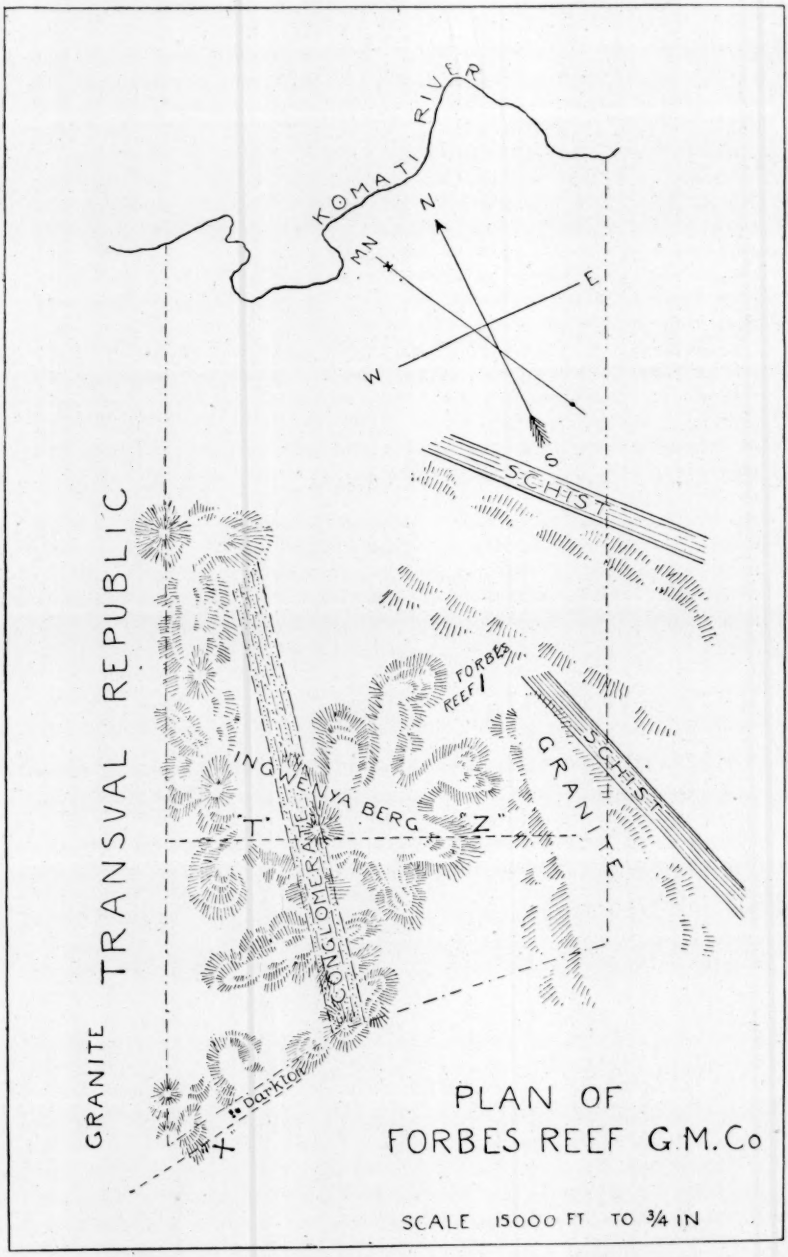


FIG. 1.—Mr. Ryan's Plan of the Forbes Reef Gold-mining Company's Property, Swaziland.

both from his own knowledge and from communication with Mr. Ryan, who has a clear notion of the structure of the district, as shown by his plan and sections.

In the course of his researches in the tin-bearing gravels of the Embabaan, Mr. Ryan found some very fine stone implements, made and used by earlier occupants of the country. He also discovered the rock, *in sitû*, of which these ancient tools were made. These large South African implements have been described and illustrated in the Journal of the Anthropological Institute, new series, vol. i (1898), pp. 48–54, pls. iii and iv. They consist of varieties of the grey and black lydianized and mylonized grit and breccia of Section T, division c, which passes along near the middle of the Ingwenya Berg, as a constituent of the western limb of the great syncline in that mountain range.

The chief feature observable in Mr. Ryan's plan of the Forbes Reef Gold-mining Company's property, Fig. 1, and in the Sections, Figs. 2 and 3, is the occurrence of a great syncline of various schistose rocks in the Ingwenya Berg. This is traversed by Sections Z and T, from S.S.E. to N.N.W. (70,000 feet, about 13¼ miles), and further to the south-west by the nearly parallel Section X (on the Ryan Tin-mining Company's property), which repeats some part of Section T, and reaches further to the west into the Transvaal (see Fig. 1).

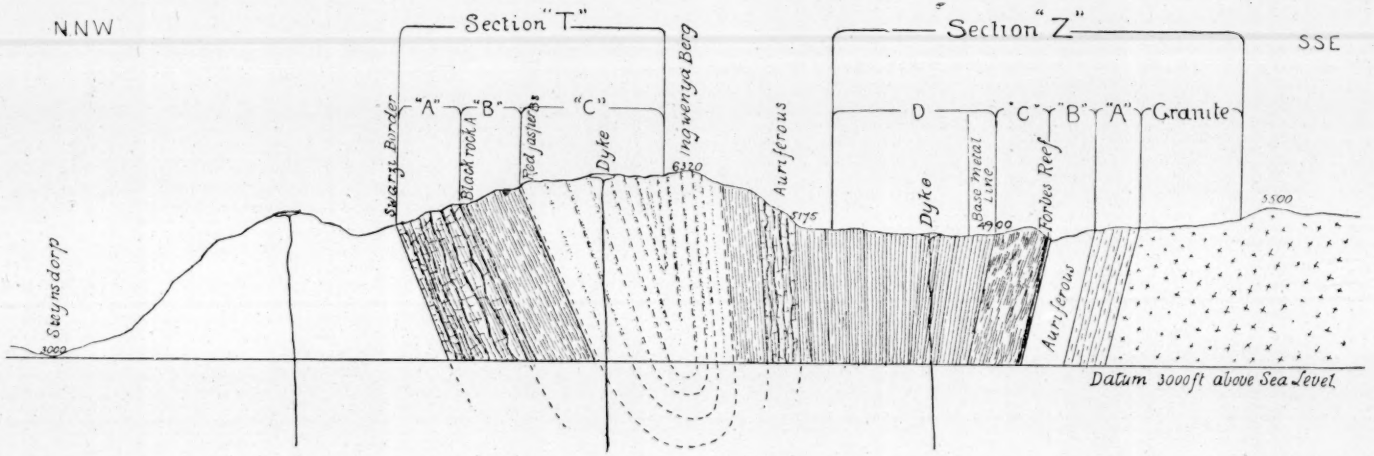
Section Z (Fig. 2), reaching from the granitic hills on the S.S.E. border of the Forbes Reef Gold-mining Company's property, passes across the Ingwenya Berg, and is continuous on the N.N.W. with the Section T (Fig. 2), which reaches the western limit of the same property, conterminous with the Transvaal.

Section T is continuous on the N.N.W. with Section Z, and meets the Transvaal border at about 63,750 feet (about 12$\frac{1}{2}$ miles) from the south-western angle of the Forbes Reef Gold-mining Company's property.

Section X (Fig. 1), running west and east, is 65,625 feet (about 12½ miles) south of Section T, and at an angle of 45° with it, and parallel with a part of the south boundary of the Forbes Reef Gold-mining Company's property, which divides it from the Ryan Tin-mining Company's property.

Between Sections Z and T a narrow space, about 5,655 feet (more than a mile) broad, on the N.N.W. half of the Ingwenya Berg and passing nearly north and south, is coloured yellow and marked "conglomerate" on Mr. Ryan's plan of the Forbes Reef Gold-mining Company's property (Fig. 1). This band corresponds with the division c of Section T, about 5,000 feet broad (specimens Nos. 83–96), belonging to the western limb of the local syncline. The highest part of the Berg (6,321 feet) is a prominent part of the eastern limb. This "conglomerate" evidently passes through the southern boundary of the Forbes Reef Gold-mining Company's property. Its northern extent is not defined on the plan further than 22,500 feet (about 4¼ miles) north of Ingwenya Berg summit. The division c of Section T consists of hard grit and breccia of lydite,

FIG. 2.—Section along the line on Map, Fig. 1, marked T-Z, in the Forbes Reef Gold-mining Company's Property, Swaziland. (See ante, p. 106.)



quartz-rock, and some jasper, with siliceous cement. Some of the materials of this rock have been greatly squeezed, crushed, and mylonized.

If the strata of Section T be continued with the same strike as that shown by the "conglomerate" band (which in Mr. Ryan's plan of the Forbes Reef Gold-mining Company's property passes obliquely across the Ingwęnya Berg on the western side of the summit, Fig. 1), through the Section T towards X, the strata T B may be found in X G, and T A in X F, as the latter section trends westward; and, further, X D, C, B, and A continue the succession on to the granite in the Transvaal. These beds in Section X (Fig. 3) dip to the east, at a high angle; and are evidently not only conformably continuous with the "conglomerate" band of the plan (that is, the breccia of T C, Fig. 2), but with the whole of the great syncline.

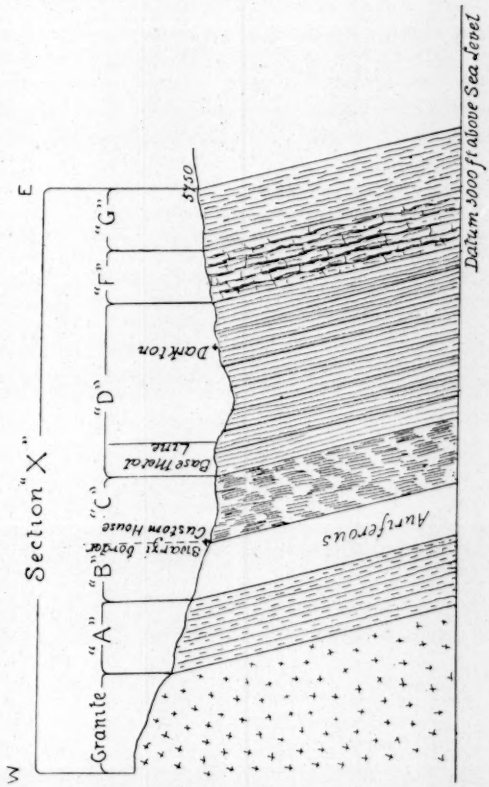


Fig. 3.—Section along the line marked X on Map, Fig. 1, in the Ryan Tin-mining Company's Property, Swaziland. (See ante, p. 106.)

A part of the eastern limb of the syncline is not represented by specimens; but in the general section (not given here) it is shown to be more vertical, and consequently more compressed. Moreover, it is immediately succeeded on the S.S.E. by a repetition of T B and

T A, as parts of the same syncline. Further on, Section Z, divisions D, C, B, and A, conformably succeed, the last resting against gneiss and granite.

In Section X, its eastern divisions G and F are the equivalents of T B and A, and these are succeeded by the equivalents of Z D, C, B, and A, in succession, as far as the granitic rocks on the west. It thus constitutes the western portion (about 15,500 feet, less than three miles) of the syncline, the diameter of which is about 45,000 feet, or about 8½ miles.

The strike of the schistose rocks on the eastern side of the syncline of Ingwenya Berg, if continued, would lead them into the Kaap River Goldfield, as part of Schenck's Swaziland schists.

Mr. Ryan has discovered a place where the natives in ancient times made their stone implements by chipping, the relics of flakes being there in abundance. This locality is on the north-west face of the Makonjwa Range, which is a northern continuation of the Ingwenya Berg. It is shown in Mr. Ryan's map of the district as being on the eastern border of the Kaap River Goldfield, and about six miles south-east of Barberton, where more or less dressed flakes of black siliceous schist, together with others, have been found.

Western half of the syncline.

SECTION X (Fig. 3).

A, 97–103. Micaceous, hornblendic, and actinolitic schists ... ... = Z A

B, 104–106 and 124. Talcose schists. 'Auriferous' ... ... = Z B
Swazi Border, Custom House.

C, 107–111. Quartz-schist and gneiss ... ... = Z C

D { Base metal line.
{ 112–123. Quartzite and quartz-schists, some talcose ... ... = Z D

F, 128–131 and 125–127. Micaceous quartz-schist, phyllite, and hæmatite ... ... = T A

G. (No specimens.) ... ... = T B

SECTION T (Fig. 2).
Swazi Border.

A, 38–56. Black siliceous schist (lydite), banded with quartz-schist, argillaceous schist, talcose, micaceous, and hornblendic schists = X F

B { Black rock, marked 'A.'
{ 57–82. Argillites, siliceous schists (some black), micaceous schists, and banded quartzite, jasper, and hæmatite ... ... = X G

C { Red jasper, marked 'B B.'
{ 83–96. Siliceous grit and breccia, lydianized = "conglomerate" band in Fig. 1.

Eastern half of the syncline.

Here is a repetition of T C, T B, and T A, in part of the eastern limb of the syncline.

SECTION Z (Fig. 2).

D { 35, 36, 17–19, 37, and 34. Talc-schist and quartzite ... ... = X D
{ Base metal line.

C, 31, 16, 32, 33. Micaceous schist and decomposed gneiss... ... = X C
Forbes Reef—auriferous.

B, 7–10, 30, 12–15. Serpentine and serpentine-schists. 'Auriferous' = X B

A, 4–6 and 11. Quartzite and mica-schist (No. 4) ... ... = X A

1, 1, 2, 3. Gneiss (fine- and coarse-grained) ... ... = X 'Granite.'

The illustrations accompanying Mr. S. Ryan's specimens are :—

1. Map of the country between the Transvaal and the Indian Ocean from 25° 30′ to 27°.
2. Section across the country from Steynsdorp to the Indian Ocean.
3. Fig. 1.—Plan of the Forbes Reef Gold-mining Company's property in Swaziland, including the lines of Sections Z and T, and Section X in the Ryan Tin-mining Company's property at Darkton. (See p. 106.)
4. Fig. 2.—The Sections T and Z in the Forbes Reef Gold-mining Company's property. (See p. 108.)
5. Fig. 3.—The Section X in the Ryan Tin-mining Company's property (p. 109).

III.—The Age of the Vale of Clwyd.

By A. Strahan, M.A., F.G.S.

FOR many years the New Red Sandstone of the Vale of Clwyd was shown upon the maps as resting generally on Carboniferous Limestone, but frequently overlapping it so as to pass on to the surrounding Silurian strata. No faults were indicated, and the structure of the Vale, as represented, suggested that a pre-Triassic syncline here existed, within which a remnant of Carboniferous rocks had been preserved from denudation and which had formed a bay in the Triassic coastline.

In 1880, however, when Sir A. Ramsay was engaged upon the second edition of his Memoir on North Wales (vol. iii of the Memoirs of the Geological Survey), doubts had arisen respecting the correctness of the map, and I received instructions to make a preliminary examination of the margins of the Vale. This I did in company with Mr. Aveline, and the results were used by Ramsay in writing chapters xxxii and xxxiii of the memoir referred to.

Subsequently the resurvey of Flintshire on the six-inch scale, which was then in progress, was carried so far as to include the whole of the Vale except its extreme southern end and a portion of its western side lying south of Denbigh. The western side north of Denbigh was surveyed by Mr. Tiddeman, and the remainder by myself. A description of the geology appeared in the Memoir on Rhyl, etc. (1885), so far as related to Quarter-sheet 79 N.W., and in the Memoir on Flint, etc. (1890), so far as related to Quarter-sheet 79 S.E. The Quarter-sheets 79 S.W. and 74 N.E. were also brought up to date in 1895, so far as was possible without a complete resurvey.

Briefly stated, the results of the resurvey went to show :—That two subdivisions of the Carboniferous system were represented in the Vale, namely, the Carboniferous Limestone and some purple sandstones and shales lying conformably upon it[1] :—That these strata

[1] Although lying outside the subject of the present paper, the history of these strata deserves a brief notice. They were first distinguished from the Trias in 1865 by G. Maw (Geol. Mag., Vol. II, pp. 380, 523, and Rep. Brit. Assoc. for 1865, Sections, p. 67), but erroneously described as Permian. In the same year D. C. Davies suggested that they would probably prove to be Carboniferous (Geol. Mag., Vol. II, p. 476). During the resurvey of 1881 it was readily seen that the purple measures lay directly and conformably upon the limestone, but were unconformably overlapped by the Trias. They were known, moreover, to contain thin seams of coal, and to include strata indistinguishable in tint or character from Coal-measures. Their

were faulted down against the Silurian rocks all along the eastern side of the Vale by a great compound fracture, to which the name of "Vale of Clwyd Fault" seemed suitable, and were repeatedly displaced on the western side of the Vale by a series of what seemed to be branches from that great fracture:—That the Trias nowhere overlapped the whole of the Carboniferous, as formerly supposed, but generally rested upon the purple strata; though it frequently overlapped them and a considerable part of the limestone also, it was never directly in contact with the Silurian rocks, except where faulted against them:—That the faulting was certainly in part post-Triassic, but that much of it had been accomplished in pre-Triassic times.

In the years 1897–98 Mr. Morton[1] published the results of a detailed examination of the whole Vale, including those parts to which the six-inch resurvey had not extended, and added not a little to what was already known in a district where the prevalence of Drift and scarcity of sections makes every observation important. He concluded that the faults were wholly post-Triassic, and remarks that he "failed to find a satisfactory section showing the Trias overlapping any portion of the Carboniferous Limestone or the Purple Sandstone," and further on says that "the assumption that the faults are partly of pre-Triassic age and partly post-Triassic seems to arise from a lingering faith in the old Map of 1850, and

Carboniferous age, therefore, was no longer left in doubt, but there remained the difficulty that they were quite unlike the rocks which lie next above the limestone in East Flintshire.

The East Flintshire Millstone Grit is of an abnormal type, for though in the southern part of the county it consists of quartz-grits and conglomerates of the usual character, towards the north it passes almost wholly into chert. With neither of these types had the purple measures anything in common, but after some little search I was able to match them exactly in some micaceous sandstones and shales which lie between the Millstone Grit and Middle Coal-measures near Mold, and which may be supposed to belong to the Lower Coal-measures of other regions. Subsequently, when the mapping showed that the Millstone Grit locally thinned out in Mid-Flintshire ("Geology of Flint," etc., p. 53), leaving the supposed Lower Coal-measures in direct superposition to the limestone, I attached more importance to the lithological affinities of the purple strata, and concluded that they also were probably younger than the Millstone Grit (ibid., pp. 33, 69).

Mr. Morton, however, states that "the Purple Sandstone and Shale, coloured as Coal-measures on the Map, are really on the horizon of the Upper Black Limestone of Prestatyn and the Arenaceous Limestone of Mold, subdivisions which do not belong to the Coal-measures" (Proc. Liverpool Geol. Soc., 1897–8, p. 280). This is true in the sense that they rest upon the "Upper Grey Limestone" of his classification, but it is not the case that they represent or pass horizontally into the black limestones. These latter are strictly local in their development, and thicken and thin rapidly in East Flintshire. In South Flintshire they are represented by alternations of sandy limestones and sandstones, and in a corresponding position in the south end of the Vale of Clwyd strata of this character, though poorly developed, are seen dipping beneath the purple beds.

This poor development, the absence of the black limestone, and the local thinning out of the Millstone Grit, may be taken with the general attenuation of the limestone as indications of a Carboniferous shore-line at no great distance westwards, the only evidence we possess that Snowdonia had commenced existence as an elevated region in Carboniferous times.

[1] Proc. Liverpool Geol. Soc., 1897–8, pp. 32 and 381.

a reluctance to supersede it by an entirely new explanation of the structure of the Vale." I would point out, however, the map of 1850 was in fact superseded by an entirely new explanation, the same which is now adopted almost in its entirety by Mr. Morton. Of this he had full opportunity of judging both from the publications on the district and from periodical inspections of my field-maps, while he was completing his field-work. We differ only on the theoretical question whether the faulting is in part pre-Triassic. That my reasons for believing it to be so have not carried conviction, makes me fear that I have not stated them with sufficient clearness.

The displacement of the Vale of Clwyd Fault, which may be anything over 1,500 feet, is usually effected by two or three parallel fractures, rarely more than 100 yards apart. The fault runs somewhat west of north on the whole, but becomes nearly north and south at its southern termination. Where there are three fractures, as often happens, the first throws limestone against Silurian, the second throws the purple beds against the limestone, and the third Trias against the purple beds. In other cases the Trias is faulted against the limestone or even directly against Silurian rocks. On the west side of the Vale the strike of the Carboniferous rocks is not along but across it. Thus they would close it in at Denbigh were they not thrown down by a large north-and-south fault, which shifts their outcrop a mile and a half southwards, and it is by several repetitions of this structure that the Vale has been carried its total length of 20 miles into the heart of the Silurian uplands. These faults, having a general north-and-south direction, presumably run into the Vale of Clwyd Fault northwards. That they were contemporaneous with it is suggested by the fact that they also obviously break through the Trias.

Although I do not know of any case where this arrangement of main fault and branches can be exactly matched, the Vale of Clwyd Fault belongs to one of the most consistently developed systems in the British Isles. The system is characterized everywhere by its N.N.W. direction. In West Cumberland the red rocks are repeatedly shifted by such faults, while they pass quite undisturbed over some huge fractures ranging S.E. to N.W. in the Carboniferous strata. The Pennine Fault, to which the Vale of Eden owes its existence, has the N.N.W. direction, and is also partly, at least, of post-Triassic age, though it follows the line of a far larger pre-Triassic dislocation. The Dent Fault is presumably of the same age. In South Lancashire the Triassic outcrop is repeatedly shifted by faults of this direction, while in North Wales there is a well-marked system of N.N.W. faults or 'cross-courses,' which are believed to be of later date than the east-and-west faults or 'lodes.'[1] The same relation holds good in Cornwall also between the cross-courses and lodes.[2] In Worcestershire and North and South Staffordshire the N.N.W. post-Triassic system is well exhibited, and includes one fracture injected

[1] "Geology of Flint, Mold, and Ruthin."

[2] De la Beche, "Report on the Geology of Cornwall, Devon, and Somerset," 1839, pp. 366–7.

with basalt of a Tertiary type.[1] In South Wales recent work has shown that the N.N.W. faults which intersect the coalfield with curious regularity are of later date than the east-and-west folds, and are in part, at least, post-Liassic. Lastly, in Somerset and Devon a similar set of faults is known to be of post-Cretaceous age, while the great disturbances and overthrusts in the Carboniferous rocks of those counties are pre-Triassic. Assuming that all these N.N.W. dislocations were contemporaneous, so far as regards their post-Triassic effects, we still can prove no more than that they are post-Cretaceous. It is probable, however, that they are principally of Miocene age, and due to the same movements which led to the fracturing of the north-west part of the British Isles and the injection into the cracks of an almost infinite number of basaltic dykes.

That the Vale of Clwyd Fault is also, in part at least, of post-Triassic age, is certain from the fact that the Trias is repeatedly shifted by both it and its branches. It is obvious, moreover, that that rock must have been tilted along the line of fault, for the current-bedding planes dip at 50° or 60° from the fault, an impossibly steep angle to be due to deposition in either air or water. The existence of pre-Triassic disturbance can therefore only be determined by eliminating the post-Triassic movements and ascertaining the position which had been assumed by the Carboniferous rocks in pre-Triassic times. This can be done by observation of the overlap of the Carboniferous by the Triassic strata.

In the north and north-eastern part of the Vale we know nothing of the relations of the two formations beyond the fact that Coal-measures, dipping steeply to the north-west, are thrown by the Vale of Clwyd Fault against the limestone at Dyserth and Meliden, while two boreholes out in the plain at Rhyl and Prestatyn proved red sandstone believed to be partly of Triassic and partly of Carboniferous age.[2] Though we have no clue to the exact origin of the measures there proved, it is obvious that they are not likely to be low down in the Millstone Grit, while they may well belong even to the Middle Coal-measures.

At Bodfari, on the other hand, it appears that the limestone must have been directly exposed to the Triassic waters, for it displays the partial conversion into hæmatite which is so commonly developed under such circumstances. The limestone, moreover, belongs to the lower part of the formation, which suggests that not only the Coal-measures but nearly all the limestone itself was here overlapped. The evidence, however, is more suggestive than conclusive.

Further south we find a narrow strip of the purple beds faulted in between the Trias and the limestone, while at Plas Draw there was in 1881 a section showing Trias unconformably banked against a little cliff cut out of purple beds. The Triassic overlap diminishes therefore from Bodfari southwards, and it seems to be in consequence of this that there is so considerable an exposure of the purple strata in the Llanbedr Farm Dingle.

[1] W. W. Watts, Proc. Geol. Assoc., 1898, p. 399.
[2] "Geology of Rhyl," etc., p. 27.

Thence to the south end of the Vale we get repeated exposures of the purple strata, and no positive proof of their being anywhere wholly overlapped. Even here, however, there would have been no reason to doubt the existence of a great unconformity, for I noticed that the lower beds of the Trias, in a little escarpment at Garth-gynan, contained bands made up apparently of the resorted material of the purple beds with what seemed to be small fragments of Silurian shale. The occurrence of such fragments would at once prove that the Trias overlapped the whole of the Carboniferous system, and would demonstrate the existence of a pre-Triassic syncline, but I was unable to determine them with certainty.

At the extreme southern end of the Vale the synclinal arrangement of the Carboniferous strata is fully revealed. The limestone dipping north-west at Tyn-y-berllan swings round to an easterly dip near Bryn-ffynnon, and in the trough thus formed encloses a small thickness of the purple beds. The Trias comes on close by in the line of the syncline, but appears to be nearly horizontal, while the Carboniferous rocks are more or less tilted. The same remark holds good of the two formations east of Llanfair Dyffryn Clwyd, but the observations are too scanty to be conclusive.

More importance attaches to the relations of the Trias to the limestone along the flanks of the hills west of Llanfair, for in following the base of the Trias along these hills we recede from the axis of the syncline, and at the same time find the Trias creeping across the edges of zone after zone of the limestone, until near Ruthin half that formation has been overlapped. This overlap shows that, whatever synclinal structure the New Red Sandstone may possess, there was a still more pronounced syncline in the Carboniferous rocks, and that it coincided with the existing syncline of the Vale of Clwyd. Between Ruthin and Denbigh the Trias is believed to rest generally on the limestone, but in four places the purple beds peep out between the two. At Llanfwrog they are thrown against the northern termination of the Coed Marchon limestone-range by an east-and-west fault, which is possibly of pre-Triassic age. At Bachymbyd they come out again for a short distance, and with the limestone below them and the Trias above are thrown against Silurian rocks by the Llanrhaiadr fault. The Trias, however, extends to no great depth near the fault, for the stream at Llanrhaiadr, below the waterfall caused by the fault, has cut down to the purple beds, giving the third of the four exposures referred to. The fourth exposure occurs at Pont Ystrad, and occupies exactly the same position relatively to the Denbigh fault as the Bachymbyd exposure occupies to the Llanrhaiadr fault.

Lastly, we have the large area of purple sandstones and shales which were found by Mr. Tiddeman to extend out into the Vale as far as St. Asaph.[1] There, again, the purple beds survived pre-Triassic denudation in considerable thickness.

From this rapid traverse of the sections along the base of the

[1] "Geology of Rhyl," etc., p. 25.

Trias we see at once that it is quite as unconformable to the Carboniferous rocks in the Vale of Clwyd as it is everywhere else. It certainly overlaps much of the limestone in places, and it certainly reposes upon strata some hundreds of feet above the top of that formation elsewhere. We see further that the greatest thickness of Carboniferous rocks lies in the synclinal trough under the Trias, and that along the margin of the Vale the purple beds are usually on the point of being overlapped. These facts can only be explained on the supposition that the Carboniferous rocks were bent into a syncline, and preserved from denudation thereby, in pre-Triassic times. From the intimate connection between the syncline and the faulting it would seem almost certain that the latter also must have been partly pre-Triassic, and that the post-Triassic disturbances only instituted fresh movements along old lines.

In this, as in other respects, the Vale of Clwyd repeats on a small scale the features of the Vale of Eden. Mr. Goodchild, writing on this subject in 1897,[1] states of the Vale of Eden that "there can be no doubt that, so far as the faults are concerned, they acted as planes of weakness again and again during several geological epochs. Some, probably the majority, are certainly of pre-Carboniferous age; but it can easily be shown that, once a fault has arisen, newer rocks at first deposited continuously across its outcrop, have been dislocated over the older fractures, sometimes in directions of movement the opposite of the original." In the same chapter he informs us that post-Carboniferous (pre-Permo-Triassic) faults followed the direction of pre-Carboniferous line of fracture, and that "the differential movements accompanying the upheaval [of the Permo-Triassic strata] were locally repeated yet again over the older lines of fault."

The overlap of the Carboniferous by the Triassic rocks gives the best proof I know of the pre-Triassic movements in the Vale of Clwyd, but I may add that a strong impression was left in my mind after seeing all the sections that the fracturing and displacement of the Trias was on a small scale compared with that which threw the Carboniferous in a fragmentary condition against the foot of the Silurian ridge.

With the great pre-Carboniferous movements of North Wales the Vale seems to have no connection whatever. The Upper Silurian strata were bent into a multitude of small folds, roughly cleaved, and subjected to vast denudation before the Carboniferous epoch commenced. It is true that they assume a broad synclinal arrangement between the Dee and the Conway, but, as pointed out by Ramsay, the synclinal axis does not coincide with the Vale of Clwyd, but lies considerably to the west of it.[2] The dip of the Carboniferous Limestone, moreover, both in East Flintshire and Denbighshire, is sufficient to carry it clear of the tops of the Silurian hills wherever

[1] Geological Survey Memoir on Appleby, etc., p. 33.
[2] Memoirs of the Geological Survey, vol. iii (1881), p. 307.

the two formations are not faulted together, nor in the rocks themselves is there any indication of the immediate neighbourhood of a shore-line, or of a land-barrier having separated the Flintshire from the Denbighshire areas. Had such a feature as the Vale existed when the Carboniferous epoch commenced, the Basement Conglomerates of that formation would have certainly revealed it. As a fact, they are almost wholly absent along the Flintshire escarpment and along the east side of the Vale of Clwyd, and though they appear in an attenuated form at the north-east end of the Vale and extend continuously from the south-west end near Llanelidan past Denbigh and Abergele to Colwyn Bay,[1] they always tend to develop in thickness and in coarseness of conglomerate northwards and westwards, but neither in the Vale, nor along it, but away from it.

The history of the Vale of Clwyd commenced, therefore, by the formation of a deep faulted syncline during the great disturbances which affected all the British area at the close of the Carboniferous epoch. During the Continental period to which those disturbances led, the Carboniferous rocks were deeply denuded from the anticlinal regions, but preserved in the synclinal folds. Upon the faulted and tilted remnant thus left in the Vale of Clwyd, the New Red Sandstone was overspread, probably overlapping it, though how far no evidence has been left to show. Long subsequently, not improbably in Miocene times, a new and widespread disturbance caused renewal of movement along the old fractures, and threw the New Red Sandstone also into the form of a faulted trough, at the same time that it must have emphasized the already existing syncline in the underlying Carboniferous strata.

REVIEWS.

I.—Wachsmuth and Springer's Monograph on Crinoids.[2]

Sixth and Final Notice.

WE have now considered the Introductory and Morphological Parts of this great work. But these, though of more general interest, serve but as foundation for the main structure, the systematic account of the North American Camerate crinoids. This Systematic Part occupies 695 of the large quarto pages, a bulk that may astonish those who regard the Crinoidea as quite a small class. Really the system of the crinoids is assuming considerable proportions; he who would master it must know the literary history of nearly 450 names, and must be prepared to classify some 250 valid genera. Moreover, the number of known genera and species increases with alarming rapidity; 13 genera and 250 species were described as new during 1896 and 1897.

[1] Quart. Journ. Geol. Soc., vol. xxxv, p. 268 (1879).

[2] The North American Crinoidea Camerata. By C. Wachsmuth and F. Springer. Mem. Mus. Comp. Zool. Harvard, vols. xx and xxi, containing 838 pp. and 83 plates. (Cambridge, U.S.A., May, 1897.) For First, Second, Third, Fourth, and Fifth Notices, see Geol. Mag. for June, July, September, and November, 1898, and January, 1899.

The value of a work like the present depends less on the number of new forms it makes known, than on the care with which it revises previous descriptions, confirms or rejects previous proposals, and summarizes with lucidity the valid residue. It must have been a disappointment more bitter to the authors than it will be to their readers, when they found that they could not make a monograph even of so restricted a portion of the field either complete or authoritative. Long years of labour in field and study, the accumulation of literature in a Western town, the unwearying collection and comparison of specimens new and old, all were deprived of their full and deserved effect when it was found impossible to examine a number of type-specimens, and when, during the very years that the work was printing, less careful or less learned writers were grinding out descriptions of species with a truly indecent haste. It was found impracticable to alter the manuscript to any great extent after it was first sent to the printers on September 1st, 1894. Consequently it is very important that all species published elsewhere between that date and May, 1897, should undergo revision and comparison with species first described in the Monograph. Till this is done and till all type-specimens have been compared, we cannot look for a standard list of North American Camerate Crinoids.

The Systematic Part opens with a section headed 'Classification.' With the broad lines of the classification of Crinoidea here proposed I have dealt elsewhere,[1] while previous chapters of the present review have touched on minor details connected with the classification of the Inadunata and Flexibilia. On the present occasion attention may be confined to the treatment of the Camerata.

The Camerata are defined (p. 169) as "Crinoids in which the lower brachials take part in the dorsal cup. All plates of the calyx united by close suture. Mouth and food-grooves closed. The top joint the youngest in the stem." If such genera as *Actinocrinus*, *Coccocrinus*, *Idiocrinus*, *Reteocrinus*, and *Enallocrinus* are all to be included in one Order, then one could hardly have a longer diagnosis; even as it stands, it is doubtful whether the criteria can be applied throughout. For instance, it is only by stretching a point that any brachials can be said to take part in the cup of *Coccocrinus*; one would not naturally suppose that the minute and irregular interbrachials of Reteocrinidæ were united by close suture; it is doubtful whether the food-grooves of the tegmen (which are what the authors mean) were closed in *Idiocrinus*; as for the last of the characters, it may be constant, but it is said also to characterize all Inadunata.

Broadly speaking, no doubt, the essential features of the so-called Camerata are the incorporation of brachials in the cup by means of interbrachials and the rigid fixation of all calycal elements. But it is admitted (p. 154) that these characters are essentially modifications imposed on an Inadunate type, and it must further be conceded that

[1] "Wachsmuth and Springer's Classification of Crinoids": *Natural Science*, xii, pp. 337–345; May, 1898.

they arose in different branches at different times. Whereas the Melocrinidæ (*sensu* W. & Sp.) existed already in Lower Ordovician times, the rise of the Platycrinidæ is traceable in the Middle and Upper Silurian, at which time also the Crotalocrinidæ, which "represent a different degree of departure from the Inadunate plan" (p. 164), first made their appearance. The question that confronts us here, as in other Classes of living beings, is whether similarity of modification is to have more classificatory weight than admitted differences of descent. From the day when the cetaceans were first separated from the fish, there has been an increasing tendency to answer this question in the negative, no matter when or where it comes up for solution.

Even Wachsmuth and Springer admit the heterogeneous nature of their Camerata, and divide the Order into: a Typical Section, in which the lower brachials and interradials form an important part of the dorsal cup; and a Non-typical Section, in which the brachials retain the form and small size of arm-plates, and the interradials are almost exclusively confined to the tegmen. Had the 'Non-typical' genera arisen first, and had their non-typical nature been due merely to arrest of development, there might be some reason or some advantage in associating them with the 'typicals.' But the fact to be emphasized is that they have no connection whatever with the rest of the Camerata. Moreover, they are themselves a heterogeneous lot, for the Crotalocrinidæ admittedly have more to do with the Dicyclic Inadunata (more particularly, the Cyathocrinidæ) than with the Monocyclic *Platycrinus* and its allies. If these statements represent a general opinion, there seems no reason why the Crotalocrinidæ should not remain in the Cyathocrinoid division of Dicyclic Inadunata, and why the rest of the 'Non-typical Camerata' should not form an independent Order (of Monocyclica). For such an Order the name Adunata is elsewhere proposed.[1]

As for the Typical Camerata, they do indeed appear to form a fairly homogeneous group; but the appearance may be deceptive. Wachsmuth and Springer do not attempt to name any subordinal divisions, but in their analysis (p. 170) they group the families under two main heads: A. Interradials ill defined; B. Interradials well defined. The former contains both monocyclic and dicyclic forms, all of which are lumped in one family—Reteocrinidæ. Group B. undergoes further division into: 1. Monocyclic (Thysanocrinidæ, Rhodocrinidæ); 2. Dicyclic (Melocrinidæ, Calyptocrinidæ, Batocrinidæ, Actinocrinidæ). Surely it is plain that the "unique and remarkable" structure of the interbrachial areas of the Reteocrinidæ represents a primitive stage in the evolution of interbrachials. Their irregular size is supposed by the authors to be due to "the intercalation of supplementary pieces" around the larger plates which "represent" "the first and second row of interbrachials in the Actinocrinidæ" (p. 184). This implies that the structure of the

[1] F. A. Bather, "A Phylogenetic Classification of the Pelmatozoa": *Rep. Brit. Assoc.* for 1898, pp. 916–923; 1899.

interradii of the Carboniferous family Actinocrinidæ is more primitive than that of the Ordovician Reteocrinidæ—a quaint paradox. On the contrary the Reteocrinid structure follows the arrangement usual in primitive many-plated areas just undergoing evolution into few-plated areas; a similar arrangement is seen in *Eocystis*, Walcott, *Deutocystis*, Barrande, in *Diabolocrinus* of the Trenton Limestone, and in the carapace of *Glyptodon*. The outward appearance of these areas may be striking, but the structure is the reverse of fundamental, and examination of the three genera referred to the family fails to show more important points of resemblance. It is not merely simpler, but more likely to bring out affinities, if we run the line between Monocyclica and Dicyclica right away through the typical Camerata. We must in fact regard the two divisions as exhibiting homoplastic modifications of distinct Inadunate stocks.

The genera are arranged in families as follows :—

1. Reteocrinidæ : *Reteocrinus, Xenocrinus, Tanaocrinus.*
2. Thysanocrinidæ : *Ptychocrinus, Thysanocrinus, Hyptiocrinus, Idiocrinus, Lampterocrinus, Siphonocrinus.*
3. Rhodocrinidæ : *Rhaphanocrinus, Archæocrinus, Diabolocrinus, Rhodocrinus, Gilbertsocrinus, Thylacocrinus, Lyriocrinus, Anthemocrinus, Rhipidocrinus, Diamenocrinus.*
4. Melocrinidæ : i. Melocrinites — *Glyptocrinus, Periglyptocrinus, Stelidiocrinus, Mariacrinus, Scyphocrinus, Melocrinus, Macrostylocrinus.*
   ii. Dolatocrinites — *Technocrinus, Corymbocrinus, Patelliocrinus, Allocrinus, Centrocrinus, Dolatocrinus, Stereocrinus, Hadrocrinus.*
5. Calyptocrinidæ : *Eucalyptocrinus, Callicrinus.*
6. Batocrinidæ : i. Batocrinites — *Batocrinus, Eretmocrinus, Alloprosallocrinus, Lobocrinus, Macrocrinus, Eutrochocrinus, Dizygocrinus, Barrandeocrinus, Aorocrinus, Dorycrinus, Agariocrinus, Acacocrinus, Habrocrinus, Desmidocrinus, Compsocrinus.*
   ii. Periechocrinites—*Periechocrinus, Meyistocrinus, Gennæocrinus, Abacocrinus.*
7. Actinocrinidæ : *Actinocrinus, Steganocrinus, Amphoracrinus, Cactocrinus, Teleiocrinus, Physetocrinus, Strotocrinus.*

For practical purposes, and with the help of the keys prefixed to each family, this grouping does well enough, and in the main it follows the broad lines of descent. At any rate it is an improvement on previous schemes. But it is the ill fate of such improvements always to disclose avenues of further progress. A few such may be alluded to.

The family Reteocrinidæ may be retained for *Reteocrinus*, but *Xenocrinus* and *Tanaocrinus* are more suitably placed at the base of the assemblage called Batocrinidæ by our authors.

The Thysanocrinidæ, to which Jaekel's *Orthocrinus* must now be added, should be relieved of *Idiocrinus*. This has not a tegmen of true Camerate type; its arms, so far as known, are not pinnulate; and its dorsal cup scarcely differs from that of *Anisocrinus*, which is a Taxocrinid. To judge from the published descriptions, it might well form a family of Flexibilia Impinnata, but is a primitive genus not far removed from Inadunata. It would also conduce to clearness to separate *Lampterocrinus* and *Siphonocrinus* as a family Lampterocrinidæ.

The Rhodocrinidæ present no difficulty, and complete the series of Dicyclic Camerata. All the rest are Monocyclic.

The Melocrinidæ of Wachsmuth and Springer comprise genera of such diverse nature that it is straining the conception of a family (as now recognized) to keep all under one name. At the same time the relations of *Eucalyptocrinus* and *Callicrinus* to some of them are confessedly intimate. On the other hand the subdivision into Melocrinites and Dolatocrinites is not regarded as satisfactory, even by its proposers. Similar criticism applies to the Batocrinidæ and its subdivisions, and to the Actinocrinidæ. None the less the reality of these large divisions should not be overlooked. I propose, therefore, to divide the Dicyclica Camerata into three Suborders—Melocrinoidea, Batocrinoidea, Actinocrinoidea—and to subdivide each of these into more limited families. The details will be given more fully elsewhere,[1] but it may be of interest to print a table for comparison. The genera are arranged in their supposed evolutionary order.

MELOCRINOIDEA.

1. Glyptocrinidæ: *Glyptocrinus, Schizocrinus, Periglyptocrinus.*
2. Melocrinidæ: *Scyphocrinus, Mariacrinus, Melocrinus, Ctenocrinus.*
3. Patelliocrinidæ: *Steldiocrinus, Macrostylocrinus, Allocrinus, Patelliocrinus, Briarocrinus, Centriocrinus.*
4. Clonocrinidæ: *Clonocrinus* (= *Corymbocr.*), *Polypeltes, Trybliocrinus* (= *Spyridiocr.*), *Technocrinus.*
5. Eucalyptocrinidæ: *Callicrinus, Eucalyptocrinus.*
6. Dolatocrinidæ: *Dolatocrinus, Stereocrinus, Hadrocrinus.*

BATOCRINOIDEA.

1. Tanaocrinidæ: *Tanaocrinus.*
2. Xenocrinidæ: *Xenocrinus, Compsocrinus, Abacocrinus.*
3. Carpocrinidæ: *Acacocrinus, Desmidocrinus, Macarocrinus, Carpocrinus.*
4. Barrandeocrinidæ: *Barrandeocrinus.*
5. Cœlocrinidæ: *Cœlocrinus* (= *Aorocr.*), *Borycrinus, Agaricocrinus.*
6. Batocrinidæ: *Batocrinus, Eretmocrinus, Alloprosallocrinus, Eutrochocrinus, Dizygocrinus, Hyperocrinus* (= *Lobocr.*), *Macrocrinus.*
7. Periechocrinidæ: *Periechocrinus, Megistocrinus, Gennæocrinus.*

ACTINOCRINOIDEA.

1. Actinocrinidæ: *Actinocrinus, Steganocrinus, Cactocrinus, Teleiocrinus, Physetocrinus, Strotocrinus.*
2. Amphoracrinidæ: *Amphoracrinus.*

The possible non-acceptance of certain details in Wachsmuth and Springer's classification cannot affect the high value of the Monograph as a storehouse of facts both new and old, while the Index and numerous tables enable one to track the contents with ease. The following new genera are described:—

Aulocrinus (p. 138, and pl. vii, fig. 9) is an ally of *Scaphiocrinus*, characterized by a forked anal tube. May not this be a persistent abnormality due to the presence of a commensal in the tube?

Tanaocrinus (p. 185), Ordovician, referred to Reteocrinidæ, but is monocyclic and apparently a Batocrinoid; in structure and in time it is well fitted to be taken as an ancestor of the Carpocrinidæ.

[1] See "A Phylogenetic Classification of the Pelmatozoa," loc. cit.

Diabolocrinus (p. 249), Ordovician, a Rhodocrinid in which the interbrachials consist of larger plates surrounded by smaller ones, the anus is at the end of a tube, and the arms bear ramules.

Periglyptocrinus (p. 277), Ordovician, a Glyptocrinid with large basals and biserial arms.

Eutrochocrinus (p. 408), with genotype *Batocrinus Christyi*, *Dizygocrinus* (p. 413), with genotype *B. indianensis*, *Lobocrinus* (p. 434), with genotype *B. Nashvillæ*, and *Macrocrinus* (p. 446), with genotype *B. Koninckı*, are closely allied genera of Batocrinidæ (*s.str.*) and all Lower Carboniferous.

Aorocrinus (p. 470) is a Devonian and Carboniferous predecessor of *Dorycrinus* and *Agaricocrinus*; the genotype is *D. immaturus*.

Acacocrinus (p. 515) is a Silurian subgenus of *Carpocrinus*, for the reception of *A. Elrodi* and *A. americanus*, but is worthy of generic rank.

Cactocrinus (p. 600), with genotype *Actinocrinus proboscidialis*, is separated from *Actinocrinus* because the arm-rami are given off in a continuous circlet around the theca, clearly separating the tegmen from the cup: the name is due to the spines on tegmen and arms; those on the pinnules imbricate over those of adjacent pinnules.

Camptocrinus (p. 779) differs from *Dichocrinus*, under which it is placed subgenerically, in the structure of the stem, which is modified like that of Hall's *Myelodactylus* and Salter's *Herpetocrinus*. *Camptocrinus* is in the Keokuk and Kaskaskia groups. Similarly modified stems were possessed by Poteriocrinidæ of Kaskaskia age. There is therefore room for doubt whether the European *Herpetocrinus* really is congeneric with the American *Myelodactylus*. No detailed account of the stem of *Camptocrinus* is given, so that one cannot ascertain to what extent it agrees with that of *Herpetocrinus*.

These genera, as well as the new species described, appear to have been established with care, and may safely be accepted. The principles guiding the authors are thoroughly in accordance with the most approved modern views. They recognize the corollary of the theory of descent, namely, that the taxonomic value of a character is in proportion to its persistence and not to its physiological importance (p. 153). Still no single character is absolutely constant, "and the larger our collections the less persistent and fixed will we find the separate characters. But if we are reasonably happy in our identifications, we may expect to find greater reliance to be put upon the correlation of characters, so that while one or more of them will show a tendency to departure, the sum of all will exhibit a predominance which will hold the form in question within the given group." Characters cannot be sorted out as of 'family,' 'generic,' or 'specific' importance, but the value or rank of a character "is proportional to the extent to which it tends to combine subordinate groups." Later on (pp. 155, 156) the difficulties attaching to the separation of species are discussed, and the necessity for appreciating stages of growth, limits of individual variation, and abnormalities is emphasized. Valuable illustrations of these difficulties will be found under *Teleiocrinus umbrosus* (p. 629) and under *Platycrinus* (pp. 652, 680, 681). The modifications due to individual growth in the latter

genus are very striking, and unless they are taken into consideration the describing of species amounts to little more than description of individual specimens. The authors base their observations largely on over a thousand specimens of *P. Huntsvillæ* collected by them, and varying from 9 mm. to 50 mm. in length of crown. So important is work of this nature, that we should have been glad of a far more detailed account and more even than the eight figures which are given of this species.

Towards the governing problems of evolution and phylogeny, our authors adopt a very cautious attitude, defended in some admirable passages (pp. 166–169). "It is," they consider, "impossible to tell, except perhaps in a very general way, which one of a number of variations marked the line of succession; or in other words which was for the time being the racial characteristic carrying all others along with it, even though many of them may seem more important." The metaphor is not commendable: at least it seems to me that the racial characteristic is usually some feature that escapes modification just because it is functionally unimportant for the time being; it follows other changes, itself unchanged.

The difficulty of deciding which are the important structures is well illustrated by *Lyriocrinus*. In the two species of this there often occur variations in "characters which have always been regarded as of the utmost significance for distinguishing families and genera," namely, the relation of interradials and basals, and the presence of an anal plate (p. 262).

Such considerations have led Messrs. Wachsmuth and Springer to shun "the primrose path of dalliance" with evolution, which they fear leads only to "the everlasting bonfire" of genealogical trees. With the modesty that comes of true learning they content themselves "with giving the general facts which [their] investigations seem to pretty well establish, and such interpretation of them as appears reasonably consistent therewith" (p. 166).

The absurd guesses that one so often sees put forward as sober accounts of phylogeny are enough to compel sympathy with the preceding remarks. But of all virtues prudence is the most uninteresting, and an author should at least have the courage of his convictions. The downfall of so many phylogenetic erections is due to the fact that they are built with their foundations in the air. It is possible, in many cases, to trace the relation of species to species, or the evolution of one genus from another: such statements often attain almost the dignity of facts, and can be checked by stratigraphy and palæo-geography. Let us begin with these, and, when the relations of genera are fairly settled, let us proceed to those of families. Thus the Orders will take shape before us, and we shall have a truer conception of what their ancestral forms were. Then we can think about the relations of Crinoidea to Cystidea and of Asteroidea to Echinoidea. And some day we may even have a right to speculate on the connection between Echinoderma and Prochordata.

Now my criticism of this Monograph is that it does not fairly

represent the beliefs of its authors in these matters. They say, it is true, "We have arranged our descriptions so as to place species which are most closely related next to each other" (p. 156). But if they have done this, it has been in a very obscure fashion; and the same principle is far from being extended to genera. The species of *Platycrinus* have been arranged in groups around certain typical species: without some such device the crowd of them would be unmanageable. But one would have liked some key to the arrangement, and an extension of the method to other genera. Consider the species of *Batocrinus*! The account begins with two allied forms from the Warsaw Limestone[1]; then come four, possibly allied to one another but not to the preceding, from the Lower Burlington of Iowa; then species from the base of the Lower Burlington of Missouri, one of them approaching *Dizygocrinus*, another near *Lobocrinus* and *Eretmocrinus*; next come two interrelated species from the Kinderhook, from which we jump to quite a different form in the Upper Burlington, then down again to its ally in the Lower Burlington; then some species, the mutual relations of which are not stated, from the Keokuk, and so back to Upper Burlington. There may be a reason for this erratic promenade; but if so, it should be stated. Again, under *Teleiocrinus*, *T. adolescens*, though said to be the species most closely allied to *Cactocrinus*, is dealt with furthest away from that genus. One begins to long for the simplicity of the alphabet. So too with the genera. *Aorocrinus* is described after *Dorycrinus*, though it is admitted to be its ancestor. Similarly *Physetocrinus* is held to give rise to *Strotocrinus*, and *Cactocrinus* to *Teleiocrinus*; but the order of treatment is—*Physetocrinus*, *Cactocrinus*, *Teleiocrinus*, *Strotocrinus*.

After the richly deserved blame which is awarded to other writers for not marking the relations of their species, one would at least expect some words of comparison to accompany each description. The frequent absence of this would not so much matter, did the authors condescend to follow the example of Linnæus and give a diagnosis of each genus and species. Instead, one has often to wade through a page or more of description, comparing it word by word with other descriptions, in order to find out the truly diagnostic characters. This is a fault so common that it may seem pedantic to notice it. So much the worse!

It must be admitted that, so far as genera are concerned, the authors have done their duty in the way of providing analytic keys. As these will probably be referred to by many students, it may be as well to point out some inconsistencies. In the analysis (p. 188) *Ptychocrinus* is said to have 10 arms; the description (p. 197) says "10 to 12"; but according to the descriptions and figures of species the rami are at least 20. In the analysis of Rhodocrinidæ (p. 215) it should be noted that *Gilbertsocrinus* has no anals, and that *Lyriocrinus* may have an anal, a correction that makes the divisions

[1] Readers of the GEOLOGICAL MAGAZINE scarcely need to be reminded that the order of the stratigraphic groups, from above downwards, is: Kaskaskia, St. Louis, Warsaw, Keokuk, Burlington, Kinderhook.

A and B of small practical use. Here also the statement "no interdistichals" is not applicable to all species of *Rhodocrinus*. In the key to Melocrinidæ we read, "C. Basals large"; but lower down *Dolatocrinus* is said to have "basals small, interbrachials few." The latter statement further conflicts with the "Interbrachials rather numerous" of p. 310. The analysis of Batocrinidæ (p. 361) obviously has *Macrocrinus* in the wrong place, since it has "Interbrachials in contact with interambulacrals at anal side only." *Eutrochocrinus* is here said to have "respiratory pores twenty," but this is not mentioned in the generic and specific descriptions. The pores of *Dizygocrinus* are said to be "not visible," but later on are described as "small."

A few other slips of like nature may be noted. On p. 205, under *Idiocrinus ventricosus*, is written "cup obtusely pyramidal"; for 'cup' read 'tegmen.' In the description of *Lampterocrinus* (p. 207) the brachials and covering-pieces are said to form a rigid tube, from which small arms are given off alternately at intervals. This statement was put forward as a hypothesis in Wachsmuth and Springer's "Revision of the Palæocrinoidea, Part II"; it does not appear to have received definite confirmation (cf. p. 209). In the description of *Siphonocrinus* (p. 210) the words "Basals five" have been omitted after "Infrabasals five." Under *Rhaphanocrinus* (p. 259) the arms are said to be "not bifurcating"; they really fork once or twice, and probably what is meant is the free portion of the arm. The basals of this genus are described as hexagonal, but they do not appear to differ from those of *Archæocrinus*, which are called heptagonal. The basals of *Lyriocrinus* are certainly heptagonal, not "hexagonal" (p. 261). *Batocrinus* (p. 366) is said to have "Arms 20 to 26," but in *B. irregularis*, *B. mundulus*, and *B. cantonensis* the rami are only 18. The horizon and locality ascribed to *B. turbinatus* and its var. *elegans* are incorrect. Hall said Burlington Limestone, Burlington, and Miller and Gurley's type of the synonym *B. lætus* came from Sedalia, Missouri. Keyes' reference in the Missouri Report was possibly copied from the proof-sheets of this Monograph, of which he was one of the artists. *Cordylocrinus plumosus* comes from Upper, not "Lower" Helderberg (p. 737). The footnote on p. 284 apparently should refer to *Mariacrinus ramosus* only, not to *M. plumosus*. *Hyptiocrinus typus*, *Idiocrinus ventricosus*, *Eucalyptocrinus Lindahli*, *Melocrinus oblongus*, and *M. Roemeri* are not new species, but were described in a preliminary notice published in the *American Geologist* for September, 1892. *Cactocrinus obesus* is not "Keyes (MS.)," but was published in *Missouri Geol. Survey*, vol. iv, part 1, p. 187, pl. xxiv, fig. 4. If that volume was issued in 1894, as stated on its title-page, the reference might have been entered; at any rate there is only one holotype, and that is not "in the collection of Wachsmuth and Springer" (p. 614).

Finally, some questions of nomenclature need attention. The genera *Dimerocrinus*, *Glyptaster*, *Thysanocrinus*, and *Eucrinus* are reduced to one, and for that the name *Thysanocrinus* is selected.

I accept the fact, but not the name. *Dimerocrinus* has sixteen years priority, and, however bad Phillips' description[1] and figures may have been, there has never been the smallest doubt as to the identity of the species. The genus must be called '*Dimerocrinus*' and the family 'Dimerocrinidæ.'

Hyptiocrinus and *Idiocrinus* are recognized as identical with the *Cyphocrinus* and *Gazacrinus* of Miller, and the type-species also are the same. Wachsmuth and Springer's names were first published in the *American Geologist* for September, 1892, and Miller's names were published in Advance Sheets from the 18th Report of the Geological Survey of Indiana. The date of these Advance Sheets, which I know were obtainable through trade channels, is here given as "October 26, 1892," and we are left to infer that the *American Geologist* was issued on September 1, or soon after. Now Miller's Advance Sheets were dated "September, 1892," and in the Second Appendix to his "North American Geology and Palæontology," issued October, 1897, Miller cited four witnesses to prove that his copies were distributed by the Survey on September 1. This might not have given Miller priority, were it not for the fact that the issue of the September number of the *American Geologist* was delayed. This I had from Miller himself in a letter dated March 14, 1894, and I confirmed it by reference to the date on which the number was received at the British Museum, namely, September 27, the usual date being about the 15th of the month of issue. So far Miller's statements have not been challenged; his descriptions are perfectly intelligible and accompanied by figures, which Wachsmuth and Springer's were without. The names *Cyphocrinus Gorbyi* and *Gazacrinus inornatus* must therefore take precedence of *Hyptiocrinus typus* and *Idiocrinus elongatus* respectively. In this connection it should be pointed out that though our authors set their imprimatur on one of Miller's figures of *C. Gorbyi* by reproducing it without comment, they have not modified their descriptions in accordance with it. Therefore their descriptions cannot supersede those by Miller, which appear to be quite accurate. Similar criticism applies in less degree to the account of *Idiocrinus*.

There is no obvious reason why Miller's trivial name '*lyonanus*' (December, 1893) should not be preferred for "*Gilbertsocrinus dispansus* (nov. sp.)," of which it is made a synonym. Miller's type-specimen has six appendages, five paired and one single, but no allusion is made to this in the Monograph.

Wachsmuth and Springer's principles of nomenclature are nowhere more difficult to follow than in their treatment of certain subgeneric names adopted by Meek and Worthen. In 1865 those writers proposed *Uperocrinus* (which we should nowadays write *Hyperocrinus*) as a subgenus of *Actinocrinus*, with type-species *A. piriformis*. This and a few allied species are now formed into "*Lobocrinus* (nov. gen.)," with genotype *Batocrinus Nashvillæ*. Such action at least demands the justification which is nowhere attempted. In 1865 Meek and Worthen proposed another subgenus

[1] Wachsmuth and Springer are wrong in saying the species were "not described" (p. 191).

under the name *Sphærocrinus*, which, being preoccupied, was changed into *Cœlocrinus* on January 1, 1866. The genotype was *A. concavus*, a fairly characteristic species of what Wachsmuth and Springer now wish to call "*Aorocrinus* (nov. gen.)." The name *Cœlocrinus* does not conflict with *Cœliocrinus*, White, 1863, and it preceded Salter's *Cœlocrinus*[1] by some months; it must be adopted. In 1881 Wachsmuth and Springer proposed a subgenus of *Macrostylocrinus* under the name *Centrocrinus*. They were aware that this name had been proposed by T. and T. Austin, but say that "the name has never been accepted by any writer, nor was it applied to any particular species by Austin himself." It was, however, accepted by Meek and Worthen in 1866 (*Geol. Surv. Illinois*, ii, p. 172), and defined with *Platycrinus gigas* as type. Since, therefore, *Centrocrinus*, W. & Sp., cannot stand, I have proposed[2] to change it to CENTRIOCRINUS, from κέντριον, 'a sting,' in allusion to the sharp spines in the type-species, *C. pentaspinus*.

Carpocrinus, Müller,[3] cannot be replaced by *Habrocrinus*, Angelin, 1878, on the ground of a single error in Müller's diagnosis; the genotype '*Actinocrinus simplex*' of Phillips has always been readily recognized.

There is far better reason to reject *Marsupiocrinites*, Phillips, 1839, previously used by De Blainville 1830;[4] it would be replaced by *Cupellœcrinus* (or *Cypellocrinus*, as we should now write it), Shumard ex Troost, 1866.

The genus *Trybliocrinus*, Geinitz, 1867, is alluded to on p. 216. There can be no doubt that this is the same as *Spyridiocrinus*, Oehlert, 1891. It may also be pointed out that *Clonocrinus*, Quenstedt, 1876, with genotype *Eucalyptocrinus polydactylus*, M'Coy (non *Clonocrinus*, Oehlert, 1879), takes precedence of *Corymbocrinus*, Angelin, 1878.

But the problems of nomenclature would keep us a thousand and one nights over this Monograph, of which it is now time to take leave. So full is it of matter and suggestion, that one might spend years in its study. This Review of it, though long, has been inadequate; but the criticism at least has been carefully considered and outspoken. Differences of opinion cannot possibly diminish the great and permanent value of the book, and everyone must hope that Mr. Frank Springer may be able before long to continue the work in which he has for many years been the colleague of Charles Wachsmuth.

These volumes are dedicated to the memory of Louis Agassiz, and it is due to the generosity of Alexander Agassiz that they appear as a Memoir of the Museum of Comparative Zoology at Harvard College. They are worthy of this intimate association with a great name.

F. A. BATHER.

[1] Appendix to A. C. Ramsay's "Geology of North Wales," First Edition, pp. 283, 284.

[2] *Rep. Brit. Assoc.* for 1898, p. 922; 1899.

[3] Ber. Akad. Wiss. Berlin for 1840, pp. 88–106; [July?] 1840. *Carpocrinus* on p. 92.

[4] See F. A. Bather, "Note on *Marsupites testudinarius*": Quart. Journ. Geol. Soc., xlv, pp. 172–174; Feb. 1889.

II.—Manual of Determinative Mineralogy, with an Introduction on Blowpipe Analysis. By George J. Brush. Revised and enlarged, with entirely new tables for the identification of minerals, by Samuel L. Penfield. 15th edition. Large 8vo; pp. x, 254, 58 tables, 375 figures. (New York and London, 1898.)

THE publication in 1896, under the editorship of Professor Penfield, of a *semi*-revised version of Brush's well-known work on the chemical determination of minerals seemed at the time a matter of doubtful policy, for would-be purchasers might well have felt inclined to wait for the next edition, containing, according to the editor's promise, a complete revision of the tables for the identification of minerals and an additional chapter on crystallography and the physical properties of minerals. As it appears, they would not have been compelled to wait long. In the present work Professor Penfield has fully redeemed his promise, and has very successfully completed the work of revision which he had undertaken.

The introductory chapters remain practically the same as in the edition of 1896. The most important changes, it will be remembered, were made in the chapter on the chemical reactions of the elements. This was extended from the fourteen pages of the older editions to ninety-four, and in its new form was eminently suggestive and instructive. The treatment was not confined to the blowpipe characters, but tests in the wet way were also given, especially in the case of elements such as those of the rare earths, which cannot be satisfactorily identified by the blowpipe alone. The minute details given in the chapter as to manipulation and the precautions necessary to ensure success should be of great service to the student in preventing him from drawing false conclusions from his experiments.

The additional chapter in the present edition on the physical properties of minerals is mainly occupied with the elementary principles of crystallography. The various types of crystals are grouped under the now accepted thirty-two classes according to the degree of symmetry they exhibit. The Millerian system of notation for the faces is explained, but no attempt is made to demonstrate the mathematical relations connecting the indices with the interfacial angles, etc. The object, in fact, has been to present the subject in as simple (and yet an adequate) a way as possible, so that the student, with the help of models, may become familiar with the more important forms presented by well-known minerals, and may thus be assisted in their identification.

The subject of mineralogy comprises so many branches of chemistry and physics that the author of a text-book, through want of space, has generally to sacrifice one part of his subject for the sake of fuller treatment of others. In the present work Professor Penfield gives us a very useful and instructive chapter on crystallography, but from limited space draws the line at the optical properties of minerals. For a description of these we are referred to his colleague, Professor E. S. Dana's "Text-book of

Mineralogy," a new edition of which has just appeared. We cannot help feeling some regret that in a work on determinative mineralogy the author has felt compelled to omit even a brief account of one of our most important aids in the identification of minerals. As a manual for the *chemical* determination of minerals, the older editions of Brush's work could be regarded as complete; but with the introduction of a chapter on crystallography in the present edition, the character of the work is so far changed that as a manual of determinative mineralogy it appears incomplete without a chapter on the optical properties of minerals.

The tables for the identification of minerals at the end of the book, originally founded on those of Von Kobell, have been brought up to date and completely rearranged: they are intended to include all well-characterized species at present known. G. T. P.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—January 18, 1899.—W. Whitaker, B.A., F.R.S., President, in the Chair. The following communications were read:—

1. "On a small Section of Felsitic Lavas and Tuffs near Conway (North Wales)." By Frank Rutley, Esq., F.G.S.

The rocks described in this communication were collected in 1877, in series, at short intervals, from a point at the mouth of the River Conway near Bodlondeb. They consist of felsitic lavas and tuffs, sometimes nodular, and generally exhibiting some variety of fluxion-structure, corrugated, or banded. A specimen showing brown bands is compared with one described by Iddings from the Yellowstone Park. What were once possibly red bands are now represented by devitrified brown glass, and the change in colour may have been due to the action of water upon the ferric oxide which originally gave its colour to the glass. Some of the rhyolites are tufaceous, and envelop fragments of rocks, some of which were originally vitreous, others lithoidal. Coarser rhyolitic tuff occurs at the northern end of the series.

2. "The Geology of Southern Morocco and the Atlas Mountains." By the late Joseph Thomson, Esq. (Communicated by the President.)

This paper gives the results of observations made under considerable difficulties during a journey in Morocco in 1888. The tract traversed is roughly triangular, the base being the Atlantic Ocean between Saffi and Agadir, and the apex the district of Demnat on the northern slopes of the Atlas, some sixty miles east of the city of Morocco. This district consists of three main sections: (1) The Coast Lowlands; (2) the Plateau in two chief steps, the northern rising to 2,000 and the southern to 5,000 feet; (3) the Atlas itself, which only begins to be a mountain-chain about thirty miles from the coast, and which ranges first east-by-north and then north-east in its central and loftiest part.

(1) The Lowlands are practically conterminous with the Tertiary deposits, among which apparently Eocene, Miocene, and Pliocene rocks are represented. The last-named consist of shelly sands 200 to 300 feet thick, gradually rising to a height of 700 feet south and east of Saffi. Their surface is often covered with the slaggy tufaceous crust described by Maw. The local presence of this crust and the porous character of the deposit elsewhere have preserved it from denudation, and thus (in the opinion of the author) its surface still presents the appearance of the Tertiary sea-bed on which it was formed. Certain quarry-like pits, one of which contains a pillar of white crystalline calcium carbonate, are supposed to be due to the explosion of steam connected with the existence of hot springs.

(2) The Plateau is underlain by three rock-formations:—(*a*) Metamorphic rocks including clay-slates, which probably underlie the whole Plain of Morocco and rise into a group of rugged hills called the 'Jebelet,' in contradistinction to the 'Jebel' or Atlas proper. (*b*) The Lower Cretaceous rocks, consisting of red shales and sandstones, the former frequently giving rise to brine springs and containing salt-deposits at Demnat in the Atlas. (*c*) The Upper Cretaceous rocks, chiefly white and cream-coloured limestones, which attain their greatest development on the Plateau.

(3) The Atlas itself is made up for the most part of the same rocks. There is a core of metamorphic rocks, which is better developed and wider at the western end of the range and narrower towards the east. Next comes the great development of the Lower Cretaceous strata, followed by a diminutive representative of the Upper Cretaceous rocks. These rocks are much broken by folding and faulting, and their structure is displayed in several sections taken across the range from Demnat westward. The first signs of glacial action were met with at Titula, consisting of moraine-like heaps of débris; elsewhere, scratched stones were found. The Boulder-deposits described by Maw were not seen either east or west of the locality described by that author; but Maw's original section was not traversed, and the present author does not offer any opinion as to the origin of the beds. Intrusive basalts penetrate the Cretaceous rocks, while porphyrites, diorites, and other igneous rocks pierce the metamorphic rocks of the central core.

II.—February 1, 1899.—W. Whitaker, B.A., F.R.S., President, in the Chair. The following communications were read:—

1. "On Radiolaria in Chert from Chypon's Farm, Mullion District (Cornwall)." By Dr. G. J. Hinde, F.R.S., F.G.S.

This paper describes the discovery of a bed of chert on the mainland, similar to that already described from Mullion Island. It was found in 1877 by Mr. Howard Fox at Chypon's Farm. Although detached blocks had been noticed in the fields, the rock had not been previously observed *in situ*. The chert is interbedded with clay-slates, and it is a dark massive rock much traversed by

quartz-veins; in some parts of it the radiolaria are preserved in an unusually perfect condition, showing their latticed structure and spines very distinctly. The radiolaria for the most part are casts only, without any definite bounding-walls, their outlines being indicated by the dark material of the groundmass, while the interior of the test has been infilled with clear silica, sometimes the cryptocrystalline variety, at others fibrous chalcedony. In the forms showing the structural details, these alone have been replaced by the opaque substance, and are thus well defined against the clear silica infilling the test. Eleven species are described, of which ten are new, while one has been previously recognized in the cherts of New South Wales.

2. "Gravel at Moreton-in-the-Marsh (Gloucestershire)." By S. S. Buckman, Esq., F.G.S.

The author describes certain gravels of Triassic débris and flints at Moreton-in-the-Marsh, with special reference to an upper bed wherein the fragments are mostly in a vertical position, some of them having their heavier ends uppermost. He theorizes that the vertical materials were the droppings from melting ice floating down a large river. This river, formed out of one or more of original consequents of the Thames system, existed before the valley of the Warwickshire Avon had been excavated. By one branch, possibly the upper Trent, it drained the Pennine range; by another, possibly the upper Severn, the Welsh hills. Ice formed in the upper waters—in these highlands—enclosing débris, and when a thaw occurred, it floated down to the lower parts of the river. The author notices certain flints obtained from the gravels. Those from the upper bed are quite unabraded; those from the lower bed have their edges worn, and in some cases battered, while sometimes they are peculiarly flaked as if artificially worked. The author surmises that the date of the gravel, if formed by a river-system as supposed, is Pliocene.

3. "On the occurrence of Pebbles of Schorl-rock from the South-West of England in the Drift-deposits of Southern and Eastern England." By A. E. Salter, Esq., B.Sc., F.G.S.

A set of twelve representative specimens, consisting essentially of quartz and tourmaline, have been looked over by Professor Bonney, who informs the author that they consist mainly of felspathic grits, schorl-rock, etc., similar rocks to which occur in the South-West of England. The most westerly point at which the pebbles have been detected is on Great and Little Haldon Hills, 800 feet above Ordnance datum, where they are of larger size, more abundant, and coarser-grained than elsewhere. Thence they are traced to the north and south sides of the Thames Basin, and into East Anglia at Walton-on-the-Naze, Aldeburgh, etc. There is a general decrease in height in the deposits in which the pebbles occur, in passing from west to east, and the pebbles appear to have taken two main courses —one along a peneplain west to east from Dartmoor, the other from south-west to north-east across England. The pebbles are absent

from the Weald and from the district around Bagshot, from the Hampshire Basin and its bounding hills (with the exception of the extreme south), and from the highest and presumably oldest gravels north of the Thames.

CORRESPONDENCE.

SUB-OCEANIC PHYSICAL FEATURES.

Sir,—I take up my pen with unusual pleasure on this occasion, as it is for the purpose of welcoming an adherent, and removing misapprehension from the mind of a supposed opponent. I welcome the adherence to my views of so able a physicist as the Rev. Osmond Fisher, and I hope to be able to remove misunderstanding as to my meaning from the mind of Mr. Jukes-Browne.[1]

Let me assure Mr. Fisher that I am well acquainted with the paper by the late Mr. Godwin-Austen, which he quotes at length, and that in the paper on the sub-oceanic physical features off the coast of Western Europe (at present only in manuscript) I commence my statement by calling attention to Mr. Godwin-Austen's remarkable communication to the Geological Society. I wish also to add that, in speaking of the subaerial origin of 'the grand escarpment,' he is correct in inferring that I included 'wave-action' along a coastline, as Professor Spencer has also done in his article in the Geological Magazine, January, 1899, p. 17. There is one point, however, in Mr. Fisher's letter, towards the end, which I would ask him to reconsider. It is quite true that the physical features on opposite sides of the Atlantic, now submerged, do possess a remarkable similarity (though not identity) of form; but I can scarcely suppose him to mean (as his language seems to imply) that it is in consequence of their original union on two sides of a 'rent'; an impossible hypothesis.

I shall now endeavour to reply, as concisely as possible, to the three points which have called forth rather severe criticism on the part of Mr. Jukes-Browne.

(1) As regards the term 'escarpment,' as applied to the descent leading down from the platform to the abyssal regions of the ocean, I quite admit that the term is not strictly geological as usually understood. For, although there may be portions of this long line of slope where the strata may be in such a position, and of such a character, as to constitute a true geological escarpment if under the air, yet we know so little of the rocks otherwise than by inference that we cannot pretend that this is the case. Under the circumstances, therefore, and notwithstanding the support of Professor Spencer for the term 'escarpment,' I am quite willing to recognize the force of Mr. Jukes-Browne's objections, and to drop that term in favour of 'Declivity.' This term, therefore, I intend to use, with the retention of the word 'Grand,' in my paper when published.[2]

[1] Letters, Geological Magazine, Sept., 1898, p. 429, and Nov., p. 527.

[2] An abstract of the paper will be published by the Royal Geographical Society in March; but the full paper later on, by the Victoria Institute.

(2) Next, with regard to the formation of the Grand Declivity by subaerial action, I now see where the obscurity of my language led Mr. Jukes-Browne to misunderstand my meaning when he says, "How the existence of river-made valleys can possibly prove the declivity to have been made by subaerial agencies passes my comprehension." But Professor Spencer,[1] as also Mr. Fisher, have recognized that under the term 'subaerial agencies' I included the erosive action of waves and currents along lines of cliff, whether of emerging or subsiding land, just as we see along our own coasts at the present day. In endeavouring to maintain, against some objectors, that these sub-oceanic features were produced under the air rather than under the waters of the ocean, I lost sight, for the moment, of the very obvious fact that rivers and waves were the chief agents in their formation. I hope I have now made my meaning clear; but before leaving this point, I wish to add that, although both during emergence and subsidence the action of the Atlantic waters in cutting back the coast was doubtless in operation, it seems most probable that the greatest amount of work was done during the doubtless prolonged pause which intervened when the change from the one direction of movement into the other was taking place; during this period, also, river-erosion was probably most active.

(3) The last important point on which I shall touch is with reference to the geological age of these sub-oceanic features. I have assumed that it was during the prolonged period extending from the close of the Eocene into the Post-Pliocene. Mr. Jukes-Browne asks, "Is there any reason why the formation of the escarpment and the union of Great Britain with Iceland should not have taken place in the Eocene period?" (GEOL. MAG., 1898, p. 430). Doubtless, there was an incipient uprising and shaping of our coasts at this epoch, as indicated by the discordant relations of the Lower Tertiary to the Cretaceous strata. But, while admitting with Mr. Jukes-Browne the probability of an incipient uplift, I strongly hold that it was not till the succeeding Miocene stage that the great elevatory movement of the Atlantic bed determinately set in. The powerful terrestrial movements during this and the succeeding Pliocene age, all over the European area and beyond, accompanied by denudation, need not here be insisted on. But the point which concerns our present inquiry is this: that the courses of the existing British and Continental streams having been mainly determined during these later Tertiary periods, we have a clue to the age of those now under the ocean, as they were once in physical connection with them. The subject is, however, too wide and intricate to be fully dealt with here; and I must hasten on to my last point, namely, the epoch of maximum elevation; and in answer to the second part of Mr. Jukes-Browne's question (quoted above), I reply, that whether or not this connection was established in the Eocene period, it is clear that it was continued (or repeated) in Pleistocene times, inasmuch as

[1] GEOL. MAG., January, 1899, p. 17.

the fauna and flora are identical, and both are of Recent age; a physical connection would necessitate an uplift of the ocean bed to a *minimum* extent of 550 fathoms.

As regards the epoch of *maximum* elevation, I have already given my reasons for holding that the extreme cold of the Glacial Epoch was the direct result of land elevation on both sides of the Atlantic (see my paper on "Another Possible Cause of the Glacial Epoch," Trans. Vict. Inst., 1898); under this view, it follows that the intensest cold would probably occur during the epoch of maximum elevation, namely, the early stage of the Glacial Epoch. I need not further dwell on this point, which I have attempted to deal with in the paper referred to.

But this communication has extended far beyond my original intention, and I must bring it to a close. It seems to me that this correspondence has "cleared the air," and that between the views of Professor Spencer, Mr. Jukes-Browne, and myself there is but little difference; or the difference is unimportant.

EDWARD HULL.

THE HORIZON OF *DINOCYSTIS BARROISI*.[1]

SIR,—Professor G. Dewalque, writing in your February number (N.S., Dec. IV, Vol. VI, p. 94), gently turns the Famennian beds of the Condroz right way up again from the reversed position into which an annoying slip on p. 543 of my paper had thrown them. For this friendly intervention he has my thanks, but with his main thesis I am unable to agree. The question at issue is the horizon of *Dinocystis Barroisi*; to this all the rest is subsidiary. Let us make the question clear by printing the list of the horizons of the Famennian, in descending order, as given in "Légende de la Carte Géologique de Belgique, etc.," 8vo, Bruxelles, 1896.

DEVONIEN SUPÉRIEUR.

Famennien supérieur.

Assise de Comblain-au-Pont [=Etroeungt Limestone].
Assise d'Evieux.
Assise de Monfort.
Assise de Souverain-Pré.

Famennien inférieur.

Assise d'Esneux.
Assise de Mariembourg.
Assise de Senzeilles.

This list does not imply an absolute vertical succession: it appears, for instance, that the Assise d'Evieux, with its rich flora, may be a more littoral facies of the Assise de Monfort, while the Assise

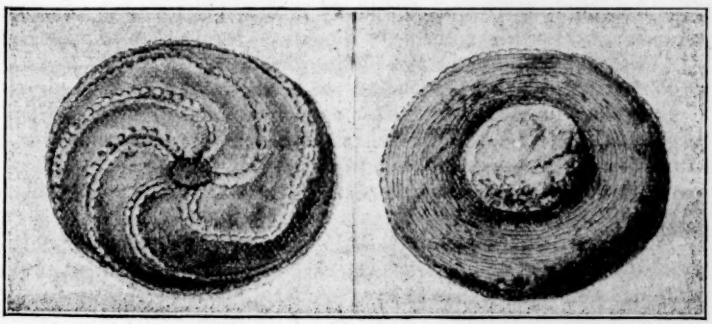
[1] See GEOL. MAG., N.S., Dec. IV, Vol. V, pp. 543-8 (December, 1898). Footnote 1 on p. 547 explained the name *Dinocystis* as derived from δεινός, *terrible*. Although this seemed peculiar, it did not occur to me that Dr. Jaekel must have intended to derive it from δινεῖν, *to whirl round*, in allusion to the marked curvature of the radial grooves. Thus regarded, the name is highly appropriate.

d'Esneux may be the arenaceous equivalent of the more shaly beds of Mariembourg.

Now my argument was that *Dinocystis Barroisi*, being what was usually called an *Agelacrinus*, was with little doubt the same as the "*Agelacrinus*" which Mourlon cited in 1881 ("Géol. de la Belgique," ii, p. 23) from "Assises de Montfort et d'Evieux"; and therefore that our specimens of *D. Barroisi* also came from these beds, "the uppermost member of the true Upper Devonian." Professor Dewalque states that, "for important reasons," he believes the *Agelacrinus* of Mourlon, 1881, to be the same species as a certain "astérie," to which Mr. Mourlon referred in 1875 as being in "collection Malaise." This "astérie," according to Prof. Dewalque, is a specimen of his *Protaster Decheni*, from the Assise d'Evieux. If these beliefs were justified, it would follow that *Dinocystis Barroisi* was not the same as the *Agelacrinus* of Mourlon, and it would be referred to the same horizon on no better evidence than an inaccurate dealer's label.

Since the validity of Professor Dewalque's criticism entirely depends on "important reasons," we should be warranted in disregarding it until those reasons have been published. But the high authority of my critic, no less than the difficulty of attributing so incomprehensible an error to the learned director of the Service Géologique de Belgique, has led me to investigate the question afresh.

The results more than justify my former inference.



Dinocystis Barroisi: photographic reproduction (reduced to $\frac{8}{9}$) of a pencil-drawing made for Mr. Mourlon in 1881, from the specimen then referred by him to *Agelacrinus*. The position of the anus was not observed by the draughtsman; it may well have been in the rather irregular interradius to the right in the drawing of the actinal surface.

Professor C. Malaise kindly informs me that the above-mentioned "astérie" is still in his collection, that it is a specimen of *Protaster Decheni*, Dewalque, and that the bed at Walcourt from which it came belongs, in his opinion, to the Assise d'Esneux (not the Assise d'Evieux, to which the type-specimen is now referred).

Mr. Mourlon most courteously sends me a drawing, here reproduced, of the specimen mentioned by him as "*Agelacrinus*" in 1881. It is not the "astérie" of Professor Malaise; it is not a *Protaster Dechenı*, or any kin thereto; but it is a fine specimen of an Edrioasteroid, as large as, and more perfect than, the British Museum specimen E 7581, with actinal and abactinal surfaces clearly shown; and it belongs incontestably to *Dinocystis Barroisi*. F. A. Bather.

British Museum (Natural History).
February 5, 1899.

OBITUARY.

WILLIAM COLCHESTER, J.P., F.G.S.

Born July 21, 1813. Died November 15, 1898.

Amongst geologists and agriculturists the name of William Colchester will always be associated with the Suffolk Crag and the Cambridge Greensand, and the exploitation of these deposits as sources of artificial manure for the farmers, and which have so largely added to the fertility of the soils, not only at home but in all parts of the civilized world. Coming of a Gloucestershire family, William Colchester was the eldest son of the late Mr. Benjamin Colchester, and spent his early life at Dedham, near Colchester, Essex. Like the late Sir Joseph Prestwich, he was educated at University College, London, and it was originally intended that he should follow the profession of an architect. In early life he travelled much in Italy, studying classic and mediæval architecture. He afterwards visited Russia, chiefly in order to learn the Slavonic language. On his return to England Mr. Colchester became identified with the late Mr. John Chevalier Cobbold, formerly M.P. for Ipswich, in connection with the importation of timber. In course of time Mr. Colchester associated himself with the ports of Ipswich and London in the carrying trade, and became a large shipowner. This led to the development of an extensive ship- and barge-building industry at the Cliff, Ipswich, which he carried on in conjunction with his other business for many years.

Some idea of the extent of this shipping business may be gathered from the fact that the fleet of vessels belonging to the firm at that time numbered more than thirty; whilst the fleet of shrimpers built and equipped by him fifty years ago which sail from Harwich are still known as 'Colchester's Fleet.' They were originally used for dredging up *septaria* from the London Clay off the port of Harwich.

The concretions known as 'septaria' form the raw material from which Roman or Portland cement was manufactured, an industry extensively carried on by Mr. Colchester fifty years ago. These dredging operations were also of great importance in deepening the channel at the mouth of the River Orwell, which, owing to the set of the tides, was liable to be obstructed by the formation of a bar across its estuary.

In 1843, after an excursion in the Crag district in the neighbourhood around Ipswich in company with his friends Colchester and

Prestwich, the Rev. Professor Henslow called the attention of the Geological Society of London to the great beds of shingle at Felixstowe, where the pebbles contained so large a proportion of phosphate of lime that he was "convinced they must all be considered as of *coprolitic origin*," and amongst which he found the fossil tympanic bones of several kinds of whales. The practical mind of William Colchester soon enabled him to turn this to account, and, after consultation with Mr., now Sir, John Lawes, a company was formed known as Lawes's Chemical Manure Company, of which Mr. Colchester was Chairman for many years.

The first area worked by this Company for obtaining the raw material for the production of artificial phosphates for agricultural purposes was in the Suffolk Crag, and extended from Felixstowe through Melton, Bawdsey, Orford Castle, to Sutton Haugh.

It was fortunate that Mr. Colchester was a geologist in addition to being a keen man of business, otherwise the wholesale destruction of the Suffolk Crag over this large area, in the search for artificial manure, would have resulted in an irreparable loss to geological science; but during the whole period the diggings were being carried on, the workmen were encouraged to collect and preserve the more interesting specimens of teeth and bones of mammalia and fishes, which form no inconsiderable part of the Crag nodules, and of which the museums of Ipswich and York, the British Museum, and many others contain abundant examples. The Crag deposits of Suffolk and Norfolk yield numerous remains of Pliocene Vertebrata mingled with derivative fossils from earlier Miocene and Eocene strata.

Since the opening of the phosphate diggings, large collections have been made by the late Mr. Edward Charlesworth, F.G.S., Messrs. Jas. Baker and W. Whincopp, of Woodbridge, Sir Joseph Prestwich, the Rev. F. Canham, and others. Mr. Colchester's own collection was lately given to the Ipswich Museum, where Mr. Canham's collection (purchased for the town by the late Sir Richard Wallace) is also preserved. The other collections are now mostly acquired by the British Museum, and, through the liberality of Mr. William Reed, F.G.S., for the York Museum.

Large works were erected at Ipswich for the crushing and grinding to fine powder of the raw coprolite, and it was no uncommon thing in those days for frequent special trains to be running laden with thousands of tons of the newly discovered mineral. In the experience of the late manager, we are told that sometimes as many as fourteen vessels, steamers, etc., have been waiting their turn to load at Mr. Colchester's works, in the Cliff Bight, Ipswich.

Mr. Colchester also acquired large chemical works at Rainham, and at his death was still a proprietor of land around that place. He was also the originator of the "Manganese Bronze Company," and was the inventor and manufacturer of steam-ship propellers for the motive-power of our great ocean-going liners.

After the cessation of the Suffolk Crag Coprolite Works Mr. Colchester became interested in the Cambridge Greensand Coprolite Diggings, and joined Mr. T. T. Ball in the Burwell Works. Some

twelve years ago he restored the old mansion of Burwell Hall, and rebuilt it much on the lines of Springfield House, Ipswich, his then residence, and thereafter he made his home at Burwell.

He was elected a Fellow of the Geological Society of London in 1857 (but did not communicate any papers to the Society); he was also elected a Member of the Geological Club in December, 1882.

Mr. Colchester married in 1840 Miss Kate Bright, a daughter of the well-known Maldon family of that name, and in 1890 celebrated his golden wedding, with all his numerous family around him. The surviving four sons and four daughters were present at his funeral with several of his grandchildren.

During Mr. Colchester's long life of 85 years he retained his mental and bodily activity almost to the last, and whilst carrying on an enormous amount of business engagements he found time to form numerous pleasant and lifelong friendships with eminent geologists, and to attend the meetings of the Societies in London and those of the British Association for many years.

PROF. H. A. NICHOLSON, M.D., D.Sc., F.R.S., F.L.S., F.G.S.

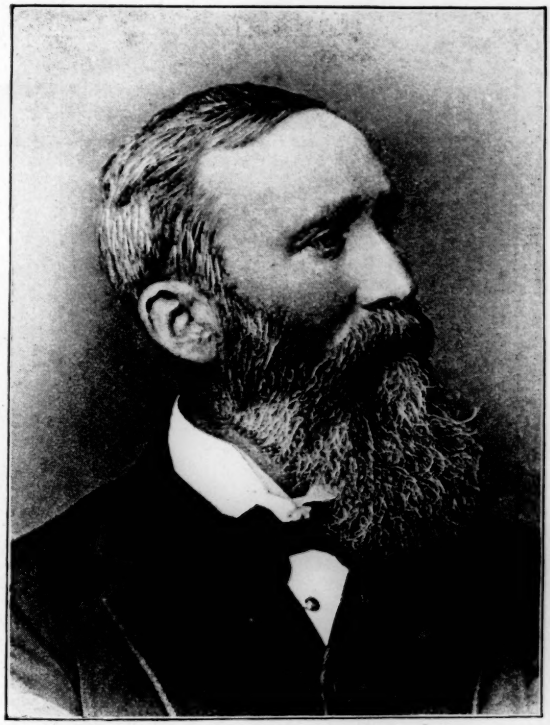
BORN SEPTEMBER 11, 1844. DIED JANUARY 19, 1899.

(WITH A PORTRAIT, PLATE IV.)[1]

HENRY ALLEYNE NICHOLSON was born at Penrith; his father, Dr. John Nicholson, was a well-known Oriental scholar, and his grandfather was President of Codrington College, Barbados. He received his early education under Francis Newman and at Appleby Grammar School, and then went to the University of Göttingen, where he worked under Professor Keferstein, the distinguished zoologist. From 1862 to 1867 he studied medicine at the University of Edinburgh, and graduated in this latter year as Bachelor of Medicine and Master of Surgery, taking first-class honours in all subjects, and obtaining the University Gold Medal for his graduation thesis, "On the Geology of Cumberland and Westmoreland"; this was subsequently published, with a dedication to his friend and teacher, Professor Harkness, of Queen's College, Cork, in whose company in the field Nicholson worked out the geology of the districts in question. In addition to the more strictly medical studies of the University he worked earnestly at the Natural Sciences under the teaching of Goodsir, Allman, and Balfour, and graduated as Bachelor of Science in 1866, receiving the Baxter Scholarship in the Natural Sciences, and in the following year he took the degree of Doctor of Science. In 1869 he proceeded to the degree of Doctor of Medicine, and was awarded at the same time the Ettles Medical Scholarship of the University, as the most distinguished student of his year in medicine.

The first position occupied by Professor Nicholson after the conclusion of his brilliant student career was that of Lecturer on

[1] For permission to reproduce this excellent portrait of our lost friend we are indebted to the editors of "Alma Mater," and to its publishers, Messrs. W. & W. Lindsay, Aberdeen.—EDIT. GEOL. MAG.



Yours very sincerely
H. Alleyne Nicholson

THE LATE PROFESSOR H. A. NICHOLSON, M.A., M.D., F.R.S.,
University of Aberdeen.

Natural History in the Extramural School of Medicine at Edinburgh, and for a short time also he practised medicine. In 1871 he was appointed to the chair of Natural History in the University of Toronto, which he held for three years. During this period he carried out successfully, not only his professorial duties, but also, at the request of the Provincial Government of Ontario, an investigation into the Fauna dredged up from Lake Ontario, and an examination of the fossils of the Silurian and Devonian rocks of the province, of which he made an extensive collection. They were described and figured in two lengthy Reports, which were published by the Government of Ontario. Professor Nicholson was also entrusted by Dr. Newberry, the State Geologist of Ohio, with the description of the Fossil Corals and Polyzoa of that State, and his Report on them, accompanied by five quarto plates of figures, drawn by Nicholson's own hand, appeared in the second volume of the "Palæontology of Ohio."

During his residence in Toronto, also, his "Manual of Palæontology" was first published, and the first part of the Monograph of the British Graptolites was issued in the same year (1872).

It will be readily understood that to a man like Professor Nicholson, with superabundant energy for original scientific work, the somewhat isolated position of Toronto would be less favourable for his special pursuit than a centre of learning within the British Isles, and it is not therefore surprising that he should have availed himself of an opportunity of returning homewards, which occurred in 1874, when he was appointed Professor of Comparative Anatomy and Zoology in the Royal College of Science, Dublin; but before he reached that city he was offered the more acceptable position of Professor of Biology in the Durham College of Physical Science and Medicine. For two sessions he lectured at Newcastle-on-Tyne, and then he accepted the offer, unsolicited on his part, of the Chair of Natural History in the University of St. Andrew's.

At St. Andrew's, where Professor Nicholson remained seven years (1875–1882), he may be said to have created the study of Natural History, and he likewise took a very active part in the extension of University teaching to Dundee, and for several years delivered systematic courses of lectures to the Ladies' Educational Association of St. Andrew's, Dundee, and Cupar. At the same time he found opportunity to carry out an immense amount of original work, more particularly in the investigation of Fossil Corals, on which subject, in addition to numerous minor papers, he published two elaborate monographs—one "On the Structure and Affinities of the 'Tabulate Corals' of the Palæozoic Period," in 1879; the other "On the Structure and Affinities of the Genus *Monticulipora* and its Subgenera," in 1881. Another important monograph of this period, produced in collaboration with Mr. R. Etheridge, jun., is that on the Silurian Fossils of the Girvan District in Ayrshire. To this original work may be added a popular volume on "The Ancient Life-History of the Earth," fresh editions of the Manual and Text-Books of Zoology, and, in 1879, a second edition of the Manual of

Palæontology, largely rewritten and so increased by the addition of new matter as practically to be considered a new book.

In the Summer session of 1878–9, Professor Nicholson delivered, at the request of the Senate of the University of Edinburgh, the course of Natural History lectures, in the stead of Professor Sir Wyville Thomson, incapacitated by illness, and he also conducted the course during the two following sessions. On the resignation of Sir W. Thomson, Nicholson became a candidate for the Chair, but Professor Ray Lankester, now the Director of the British Museum of Natural History, Cromwell Road, received the appointment, which he shortly after renounced, and the position was then given to Dr. Cossar Ewart. By many of the friends of Professor Nicholson, the slight shown to him by his own University in thus passing over his undoubted superior claims as a teacher, was attributed to the influence of a small faction, whose opposition was stirred up by the independence of Nicholson's views as to the all-sufficiency of the Darwinian theory of Evolution.

In 1882 Professor Nicholson accepted the appointment of Regius Professor of Natural History in the University of Aberdeen, which he held for the remainder of his life. In addition to his official work, he still continued his researches on fossil Corals and Monticuliporoids, and contributed several papers on these organisms to the Annals and Magazine of Natural History and to the GEOLOGICAL MAGAZINE. Some of these papers were written in conjunction with Dr. A. H. Foord and Mr. R. Etheridge, jun. But the most important original work produced at this time was the monograph of the British Stromatoporoids published by the Palæontographical Society (1886–1892). The seventh edition of the Manual of Zoology, which had been wholly recast and almost entirely rewritten, appeared in 1887, and two years later a third edition of the Manual of Palæontology, in which the additions were on a still more extended scale, was published. In this edition the first volume, containing the "General Introduction" and the "Palæontology of Invertebrates," was the work of Professor Nicholson, whilst the second was written by Mr. R. Lydekker.

Coming to a more recent period, the passing into law of the new University Ordinances in Scotland involved a large amount of extra work in connection with the reorganization of the University classes, and to this object Nicholson devoted all his available energy, to the entire exclusion of original research, which he looked forward to resuming at a future opportunity. As part of the revised system, he introduced courses of lectures on Systematic and Practical Geology and advanced laboratory work in the same science, and he voluntarily undertook this geological teaching without extra remuneration. Such practical zeal on behalf of his favourite science was not lost on his students, for the numbers attending the geological course increased from twenty when first started to eighty at the end of three or four years.

Professor Nicholson's ability as a teacher and lecturer was pre-eminent. It has been said of him by an old friend that he was

a 'heaven-born' teacher; however this may be, the writer can testify that his lectures, in the early days of his professorial career, were characterized by the same fluent delivery, without a shadow of hesitation, by the same clearness of exposition, making a complex subject appear simple to the understanding, and by the same fulness of illustration, which distinguished them throughout the succeeding years. It is not surprising that these lectures secured the attention of full classes of University students, so that discipline was never called for, and they proved equally as attractive and fascinating to the mixed general audience who heard the courses of Swiney lectures on geological subjects, which, on the appointment of the Trustees of the British Museum, were given by Professor Nicholson for several successive years (1878–82, 1890–94) at South Kensington. Although the delivery of the lectures appeared to be without effort, and as it were natural on the part of the lecturer, they involved no small expenditure of mental energy, and their success was due largely to the careful preparation bestowed on them beforehand. The various subjects were illustrated by numerous well-executed diagrams, all the work of Nicholson's own hand, and these were supplemented by chalk sketches on the blackboard drawn with ready aptitude as occasion required, and, when possible, by demonstration of actual specimens. Nothing was omitted which would be of service in elucidating the subject of a lecture, and the time allotted for it passed all too quickly, at all events on the part of those who listened.

The departments of Science for which Professor Nicholson felt the strongest predilection were Field Geology and Invertebrate Palæontology. For the study of the former, no scenes more favourable could have been chosen than those surrounding his birthplace and the home of his boyhood in the Lake District; here most of his holidays were spent, and rest and renewed vigour obtained in working out in detail the complicated geological structure and collecting the fossils from the rocks for subsequent study. As already mentioned, his graduation thesis consisted of an essay on the Geology of Cumberland and Westmoreland; but previous to this, he had already published, in conjunction with Professor Harkness, a paper of "Additional Observations on the Geology of the Lake Country," and also "On the Coniston Group," announcing the discovery of a rich Graptolite fauna in the Coniston Flags of Sedgwick. Other papers on the Green Slates and Porphyries between Ulleswater and Keswick and on the Borrowdale Series and Coniston Flags appeared in 1871 and 1877. A still more important memoir on the Stockdale Shales, in which the work and the honour were shared equally with his friend Professor J. E. Marr, was brought out in 1888. In this paper the Stockdale Shales were shown to be capable of division into a series of zones based on lithological characters and their contained fossils, mainly graptolites, and additional evidence was thus furnished of the value of these organisms as a means of comparison of Lower Palæozoic rocks of distant areas. A shorter paper by

the same authors, which appeared subsequently, dealt with the age and the fossils in the various formations in the Cross Fell Inlier.

Professor Nicholson's researches in Invertebrate Palæontology were concerned mainly with Graptolites, Corals, Monticuliporoids, and Stromatoporoids. He first devoted his energies to the Graptolites, with which some of the beds in the Lake District are crowded, and described those in the Skiddaw Slates, pointing out their close resemblance to the forms in the Quebec Group of Canada. Then followed papers on *Ptilograptus*; *Helicograptus*, a new genus; the Distribution in time of the British species and genera of Graptolites; the Graptolites of the Coniston Flags; British species of *Didymograptus*; and on the genus *Climacograptus*, with notes on the British species of the genus. These detached papers were succeeded by the first part of a detailed monograph on the Graptolites of Britain, in which the history, morphology, the nature and functions of the base, mode of existence, geological distribution, and definition of the various genera of the group were ably treated. Further work was interrupted by Nicholson's residence in Canada, and not again resumed (save in a brief but important paper jointly with Professor Marr, dealing with the phylogeny of the group, which appeared in the GEOL. MAG., Dec. IV, Vol. II, 1895); the task thus relinquished passing thenceforward into the hands of his friend Professor Chas. Lapworth. It is well known that the study of the Graptolites in the rocks of this country is very difficult on account of their compressed condition of preservation, and it required much critical acumen on Nicholson's part to decipher their characters so correctly as he has done.

The marvellous abundance and variety of forms of Corals and Monticuliporoids in the Palæozoic rocks of North America induced Nicholson to make large collections of them during his stay on that side of the Atlantic, and they furnished the materials for many years' study after his return. At the time when he began his work on these organisms, the practice of making thin sections to show their interior structure had not come into vogue; Nicholson was one of the first to appreciate the advantages of this method of investigation, so he procured a lapidary's wheel, and with unwearied diligence set himself to make thin slices, in various directions, of all the forms he studied, and then mount them for the microscope. By this means he was enabled to ascertain the real structure of the fossils, and to define their characters with a precision not previously attainable. The fresh knowledge thus obtained of the actual anatomy and systematic relations of the Palæozoic Corals and Monticuliporoids, formed the basis of the two elaborate monographs on these groups, and gave to them a special value and interest. Nicholson's determinations of the systematic affinities of some of these forms were, as he himself acknowledged, of a tentative character, and the tendency of recent opinion with respect to the Monticuliporoid group is to associate it with the Polyzoa rather than with the Corals, where it had been placed by Nicholson, but this in no wise affects the accuracy of his descriptions and figures of the various forms of the group given in these monographs.

The Monograph of the British Stromatoporoids will probably be regarded as Nicholson's most important contribution to Palæontology. The dubious character of these fossils is plainly indicated by the various conflicting opinions held about them by many leading palæontologists, and even our author found it needful to abandon his own earlier views of their nature. Nothing daunted, however, he continued his researches, and gathering his materials from the Silurian and Devonian rocks of Britain and North America, also from the Silurian of Esthonia and the Devonian deposits of Germany, he made personally more than a thousand microscopic slides from the specimens. The wonderful variety and beauty of the minute structures shown in these slides were represented in perfect detail in the drawings, made by Nicholson himself, which occupied the twenty-nine quarto plates and the woodcuts in the text of this monograph. As the result of these extended investigations, Nicholson was enabled to show conclusively that the Stromatoporoids belonged to a special division of the Hydrozoa, a relationship which had been already suspected by Lindström and Carter. It is satisfactory to know that the grand series of specimens and the microscopic sections made from them, which formed the basis of this model monograph on the Stromatoporoids, have passed into the possession of the British Museum (Natural History), Cromwell Road, and we believe that the typical specimens of Nicholson's collection of Graptolites are also in the same institution.

Professor Nicholson further took great interest in the nature of certain anomalous organisms which are concerned in the formation of some of the Palæozoic Limestones, such as *Solenopora*, Dyb., *Mitcheldeania*, Weth., and *Girvanella*, Nich. & Eth., and described and figured them very fully, and he also very satisfactorily cleared up the doubtful points relating to the structure of the genus *Parkeria*, Carp.

But Nicholson's services to his favourite sciences were not confined to original researches, for his Manuals and Text-books of Zoology and Palæontology may deservedly be considered as important adjuncts to their advancement. They are characterized by clearness and perspicuity of style, and by the number and excellence of the figures and diagrams with which they are illustrated. That they are widely used is shown by the number of editions which have been called for, and in each successive edition there is such a large amount of additional matter as to entitle the book to be regarded as new. This is well exemplified in the "Manual of Palæontology," the first edition of which was a single volume of 601 pages and 401 figures, whilst the third edition comprises two volumes, with 1,624 pages of letterpress and 1,419 figures. Only the first of these volumes, which deals with Invertebrate Fossils, is by Nicholson's own hand; it is rendered especially valuable to the practical student by the careful descriptions and figures of the microscopic structures of the different kinds of organisms occurring in the rocks, so that they can be recognized in thin sections. This volume is the most complete general work on Invertebrate Palæontology which has appeared in the English language.